

## Has Demographic Stability Come to Stay?

THE United States Commission on Population Growth and the American Future, set up nearly two years ago at the request of President Nixon and in response to the clamour for a population policy, has now reported, but should not be thought to have produced a charter for what is called Zero Population Growth. Quite properly, of course, the commission has said that it sees no economic argument in favour of continued growth of the American population—ever since the disappearance of subsistence agriculture, and the long-standing belief among primitive agriculturalists that more children make more farm hands at comparatively little cost, there have been very few reasons to believe that large families are an economic advantage to their begetters. Yet in spite of what the commission has to say about the present tendencies in the American population, and in spite of its painstaking analysis of what might be done by democratic methods to regulate the size of the American population in the years ahead, confusion will no doubt persist about the objectives of the policies that might be pursued in the future. The most perplexing difficulty of all is to know, in a country such as the United States, what size of population to aim at. The definition of a population optimum is, in particular, an illusory objective.

In the past half century, the population of the United States has almost doubled. In 1920 there were 106 million people. By 1970 there were 205 million Americans. Statistics like these have stimulated much of the anxiety which there has recently been about the future. If the population can double between 1920 and 1970, is there not at least a chance that it will double again between 1970 and, say, 2020? Fortunately, the crude statistics conceal a number of complexities which, in the United States as in the United Kingdom (see *Nature*, 234, 499; 1971), vitiate simple extrapolations like these. In the first place, the past half century has seen a remarkable reduction of mortality at all ages in the American popula-

tion. Not merely has the expectation of life at birth been steadily increased, but the chances that individuals would die in infancy, childhood and early adulthood have been dramatically reduced. One of the components of the recent dramatic growth of population has been the way in which death can now be postponed. But medical research is a field in which the principle of diminishing returns will become continually more apparent. In short, there is no reason to hope—some would say fear—that the reduction of mortality which there has been in the past half century will be repeated in the years ahead.

In the United States, as in Western Europe, the past few decades have also seen dramatic changes of fertility. On present form, the fertility of the American population is lower than, but comparable with, fertility during the Depression. In the mid-1930s, in the United States, the general fertility rate (number of births per year per thousand women) fluctuated between 76 and 79. Preliminary figures for 1971 suggest that the general fertility rate was 77.5 births per thousand women, and the signs are that this tendency will continue, with the result that the American population may in the years immediately ahead be distinctly less fertile than at any time in recent history. In other words, the report of the commission on population has appeared at a time when its recommendations are likely to be borne out by the demographic proclivities of the American population.

It remains a great puzzle to know just why the mid-1930s and the late 1960s appear to have been periods of historically low fertility. The great Depression is, for example, an insufficient explanation of the fertility minimum in the mid-1930s, for (with the neglect of the demographic consequences of the First World War) fertility in the United States declined steadily from more than 125 births per thousand women per year in 1910, and the reduction was well under way by the early 1920s, when fertility fell below 110 births per thousand women per year in 1924. Since the 1930s, the fertility of the American population seems to have increased almost as dramatically as it fell in the early decades in this century, so that by the late 1950s the American population was almost as fertile (with 123 births per thousand women per year in 1957) as in 1910. The decline of fertility since then has been uninterrupted and more rapid than in the period preceding the great Depression.

Another consequence of the size and rapidity of these fluctuations is that attempts to forecast what the population will be in the years ahead are certain to be hazardous and may, for many practical purposes, be meaningless. The United States Bureau of the Census, like analogous authorities elsewhere, has been repeatedly surprised by the rapidity of the changes which there have been in the reproductive pattern of the American population. Two years ago, in April 1970, the bureau found it necessary to supplement its previous attempts to forecast the American population, based on alternative assumptions about

### Conference on the Environment

*Nature* and the Royal Institution will hold a conference on environmental problems at the Royal Institution, Albemarle Street, London W1, from 1.45 to 5.45 p.m. on Friday, April 28, 1972.

Speakers will include Dr K. C. Dunham (Institute of Geological Sciences), Dr Kenneth Mellanby (Monks Wood Experimental Station) and Dr William Brass (London School of Hygiene and Tropical Medicine).

Applications for tickets (up to 2 per person) should be sent to: *Nature*/RI Conference, Macmillan Journals Ltd, Little Essex Street, London WC2R 3LF. Tickets will be free but applicants are asked to send a stamped self-addressed envelope.



fertility, by adding an extra series of predictions based on the assumption that fertility would be essentially that required to replace the population but, even so, by the end of 1971 it was apparent that substantial variations of the forecast might be brought about by changes in the timing of births. There is some evidence that American women, like those elsewhere, are likely in the year immediately ahead to complete their families at a comparatively early age. Even so, however, there are great hazards in the extrapolations, and predictions made in 1970 of the size of the American population in 1971 appear already to have been proved false by registrations of births in 1971, when the number of births (3,600,000) turned out to be 100,000 less than the most conservative of the predictions. Indeed, in the past few years, it has been expected that the early 1970s would see the beginning of another baby boom as the parents of the postwar burst of procreation became, in their turn, grandparents. Although the surprisingly small number of births in 1971 is likely to be exceeded in the next few years, as the large age groups now in their early 'teens succumb to parenthood, there is at least a chance that the postwar baby boom will not be faithfully echoed in the 1970s.

For reasons like this, it is, of course, perplexing to know precisely what is meant by demographic stability. There is, however, a good case for believing that numerical constancy, the literal and common interpretation of the slogan Zero Population Growth, is at once too arbitrary and too severe. The chief objection to the use of this criterion for stability is that its attainment would imply that, in the United States at present, the number of births each year should be reduced so as to match the number of deaths, at present just over two million and likely to increase only by 500,000 or thereabouts by 1990, as the youthful population of the United States gets nearer to old age. Not merely would such a reduction of the crude birth rate be impractical, for the flight from motherhood in the United States has not yet reached the point at which such a reduction of fertility could be expected naturally, but it is doubtful whether American society would welcome a 30 per cent reduction of the numbers of young children, with the depopulation of the schools six years from now and the increase in the ratio of dependants to wage earners which there would be when this year's babies were old enough to work. A better criterion of stability is that fertility should be reduced to such a point that the numbers of girl children born each year should be just enough to replace their mothers as childbearers, which, allowing for mortality and infecundity, implies that each woman should produce on the average 1.06 female children in her lifetime. The statistics imply that the American is not far short of such a goal. A more empirical but almost as satisfactory a criterion is that the number of births each year, averaged over a sufficiently long period to smoothe out previous fluctuations in the birth rate, should be constant. By this test, the reproductive excesses of the late 1950s and early 1960s in the United States have been cancelled out by the smaller number of births each year in the early 1950s and the late 1960s. In short, in spite of all the fuss which there has been about the growth of the American population, demographic stability is already within sight.

But may there not be some benefits in further reductions of fertility? This is one of the questions which the commission on population was asked to consider. One of the first things to be said is that in countries like the

United States the avoidance of large families may be a more important and a more practical goal than the detailed regulation of the fertility statistics. By now, there is plenty of evidence that the children of large families suffer all kinds of social and intellectual disadvantages which appear to stem simply from the pressure of numbers on the attention and the resources of parents. Measured IQ, for example, is negatively correlated and, although nobody will pretend that measured IQ is more than an empirical construct, the chances are that the avoidance of large families would in the long run be more beneficial for a country such as the United States than any of the eugenic schemes which have been canvassed in recent years. To be sure, large families and poverty tend to go together, which implies that attempts to make progress in this direction will require that the case for family planning will have to be carried to the least receptive parts of the American population, and it will be necessary, in the process, somehow to avoid the impression that the middle class, hitherto the zealots of population control, are out to keep the poor at bay. This is one of the many thorny issues with which, it is to be hoped, the commission on population will grapple when its detailed recommendations are published in the next few weeks. Not merely should it be made easier for women in poor families to have access to the techniques of contraception, abortion included, but steps will have to be taken to make sure that everyone is well aware of the handicaps which the children of large families must surmount.

The question of how to seek to regulate the total size of the American population is necessarily more difficult. The commission on population says quite properly that one of the most important steps which can be taken is to make sure that potential parents know what are the disadvantages of rapid population growth. For the country as a whole, there are, of course, all kinds of reasons for avoiding too great a demand by children and teenagers on communal resources. It would be a great benefit, for example, if the educational system in the United States were able to look forward to a period of numerical stability in the years ahead. And is it possible that some of the recent reduction of fertility is to be explained by such recent tendencies as the increasing cost of private education in the United States, and the increasingly apparent inadequacies of public education? The cost of medical care, the effect of which is to impress a good many people in the United States with the economic costs of increased longevity, may also have played a part. What this implies is that it may be possible to influence the childbearing habits of large segments of the American population by making them aware of the high cost of childbearing in an advanced community. The trick must be somehow to devise a way of influencing potential parents without penalizing their children, which implies that the steps which may be taken to help regulate the population of the United States should not hold up the further liberalization of medical care, especially for children, and the improvement of public education.

How ambitious should be the attempts which will be made to bring about a further reduction of fertility in the United States? It is important that too rapid a reduction could well, in due course, be counterproductive. If the number of births each year should be so sharply reduced that the proportion of dependants (old and young) in the United States should at some stage in the



future be much greater than at present, the result might well be a further stimulation of childbearing. At present, people over 62 number 25 per cent of the adult population (between 21 and 61). If by magic the numbers of children born were to decline by 100,000 in each of the next ten years, then 30 years from now the adult population of the United States would be five million less than if the number of births were constant, with the result that the proportion of people of retirement age would increase from 26 per cent (on the most conservative projection) to 33 per cent of the adult population. The economic consequences of changes like these are not lightly to be shrugged off. Just as there may be serious economic consequences of rapid growth, so too the consequences of too rapid a decline of population may be hard to stomach.

The other side of this coin also needs to be described. Although the commission on population is certainly right in saying that rapid growth is not at present desirable, the policies which it advocates entail some penalties. Thus a declining population is necessarily an ageing population, and one in which there are fewer opportunities for young people to supersede their elders in positions of responsibility. In other words, one of the social changes implicit in the commission's recommendations is that the United States will be less able in the future to exploit the energy and talents of young men and women for the transformation of organization in government and in industry. Most probably, with an ageing population, the United States will also be less able to extend its influence abroad by what amounts to the colonization of overseas industry by armies of highly trained managers. It remains to be seen whether the luxury of early retirement will be as easily come by at the turn of the century as it is at present. And there are clearly all kinds of social hazards in a situation in which there will be one retired person for every three or four productive adults. Will the retirement cities of Florida and California be the ghettos of the future? At this stage, nobody can tell with any certainty whether the compassionate forces in American society will stand up to the ageing tendency which has not set in. What does, however, follow from the commission's perceptive analysis of the American problem is that there is no point in asking questions about population optima.

## Women at the Top

IN spite of growing agreement that women can be extremely successful in highly responsible professional positions, there is pathetically little evidence of efforts to stem the complex forces which favour the progress of their male counterparts. A study by the research organization, Political and Economic Planning (PEP), of women in top jobs has just culminated with a broadsheet recommending a broadly based attack on the attitudes and traditions prolonging such a situation (*Women in Top Jobs: The Next Move*, by M. P. Fogarty with Rhona and Robert Rapoport, PEP broadsheet No. 535, £1.00). The evidence gathered during the four years of the £75,000 project suggests that the banner waving of some proponents of women's liberation will be less successful than quiet manoeuvres behind the scenes within individual organizations.

There is, however, also a recommendation for political action. The authors of the broadsheet conclude that there should be established, probably within the Departments of Employment and Productivity, and of Health and Social Security, specialized units to stimulate and direct the changes needed in family life and in employment practices. Once the necessary policies were set up, such units would fade away or be reduced to a secretariat with no more than a watching brief.

Anti-discrimination legislation also has a role to play in strengthening efforts at education and persuasion, but the broadsheet advises against an over-legalistic approach. The problems to be tackled do not usually arise from simple cases of sex discrimination. More often it is a matter of altering selection techniques and time tables to take advantage of the particular qualities women have to offer and to allow for their commitments to their families.

The PEP researchers find that it is not only attitudes towards women in top jobs that must change. Men in managerial positions are showing increasing signs of rating their families as equal or superior in importance to their work. Such a balanced approach to life has traditionally been associated more with women than with men, who have been supposed to pursue their careers relentlessly and ruthlessly above all else. This trend, combined with a growing reaction against what the broadsheet calls "the cult of overtime", should increase the professional opportunities for women. But one problem which will probably prove exceedingly difficult to solve is the need to break through "the fog of mutual misunderstanding by each sex about the interests and capabilities of the other".

## 100 Years Ago



We have received the seventh Annual Report of the Massachusetts Institute of Technology. It was established on the principle that all the studies and exercises of the first and second years should be pursued by the whole school. At the beginning of the third year, each student selects one of the following special courses of study:—1. A course in Mechanical Engineering; 2. Civil and Topographical Engineering; 3. Geology and Mining Engineering; 4. Building and Architecture; 5. Chemistry; 6. Science and Literature; 7. Natural History. These courses differ widely, but certain general studies are common to them all. It is intended to secure to every student, whatever his special course of study, a liberal mental development and general culture, as well as the more strictly technical education which may be his chief object. The course in Science and Literature, and the course in Natural History, differ from the others in having a less distinctly professional character. The former offers a sound education, based on the sciences and modern literature, and furnishes, with its wide range of elective studies, a suitable preparation for any of the departments of active life, or for teaching science. The course in Natural History affords an appropriate general training for those whose ulterior object is the special pursuit of Geology, Mineralogy, Botany, Zoology, or of Medicine, Pharmacy, or Rural Economy.

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## OLD WORLD

# What Went On at Vandenberg?

THE Machiavellian dealings which last week delayed by several days the launch of TD 1A, ESRO's biggest and best satellite, have cast serious doubts on the future of European collaboration with the USA in space. The official excuse first offered for this delay was that it was necessary to consider the impact of the launch on the schedule of future Thor Delta launches.

According to reports from California, the US Environmental Sciences and Services Administration had decided it wanted the Thor Delta rocket—one of the few now available—for a meteorological satellite. Astonishingly, NASA took this request seriously enough to postpone the launch of TD 1A, in spite of all that has been written lately affirming unconditional availability of NASA launch facilities for ESRO satellites.

By mid afternoon (GMT) on March 8, another excuse was being offered by NASA. The launch had been delayed, it was now claimed, "as a result of a technical difficulty" and NASA was conducting a review of what action to take. The result of this review was that the launch was rescheduled for 0155 GMT on March 10. Meanwhile, in Washington, no mention was made of technical difficulties and, according to informed sources, a NASA priorities study was carried out (less than 24 hours before the planned launch) to determine which satellites would receive priority on the few Delta launchers available. Sanity prevailed, and TD 1A, the satellite already mounted on the launcher, was assigned highest priority. The US meteorological satellite went further back in the queue.

After further delays caused by technical difficulties with the velocity cutoff system of the launcher's second stage, TD 1A was launched at 0155 GMT on Sunday, March 12. The European Space Operations Centre at Darmstadt reported earlier this week that the satellite went into its planned Earth orbit of 97 min. The solar paddles and antenna were also reported to be working satisfactorily, but there is no news yet of the experiments on board, as they were only switched on on Wednesday, three days after the launch. ESRO must have been relieved to have finally announced that "all mission activities and objectives are being achieved".

But although the ESRO satellite has now been accorded the facilities originally promised, the relationship between the European and American space

organizations has been strained. Proponents of the independent ELDO launcher will certainly make much of the fact that removing TD 1A from the launch vehicle was seriously considered at such a late stage. The argument is bound to be that if this is the way things are to be run, then Europe should have control over its own rockets. To be sure, this would involve much greater expense. The so far unsuccessful Europa II launcher has already cost £250 million to develop, and if it did work it could not launch half ton satellites such as TD 1A, for which the Delta launcher cost only £2.7 million. So, by continuing with its own launcher

development, Europe would end up with a "tail" more expensive than the "dog"—but at least there would be no question about who says jump.

It is hoped that no such need arises, and that the curious case of TD 1A will remain an isolated instance. But United States attempts to acquire launchers allocated to ESRO will have to be considered in the light of European contributions to the shuttle programme. The best European contribution will be obtained if the ELDO launcher is finally abandoned in the secure knowledge that NASA's facilities are available not only cheaply but also with no strings attached.

## ENVIRONMENT

# WAWF Supports Stockholm Conference

AS the Stockholm conference on the human environment draws nearer, the numbers of those who doubt its ability to do anything effective seem to grow. The June conference started out as man's "last best chance", but doubts over its machinery and scope, and more particularly the behaviour of the major powers over the inclusion of East Germany round the table, have put the effectiveness of the whole conference in question.

The latest group to express its doubts is the World Association of World Federalists. Founded just after the Second World War they advocate a world government and a world police force, but last week they assembled fifteen industrialists, scientists, politicians and academics to discuss which environmental problems must be dealt with on a world-wide, rather than a national, basis, and to indicate the steps by which decisions on these problems could be put into effect. Participants included Dr Aurelio Peccei, founder of the Club of Rome, Professor Jan Tinbergen, Nobel Prizewinner for Economics in 1969, and Dr Alexander King, Director General for Scientific Affairs OECD. At the end of their three day meeting at Windsor they issued a statement deploring the diplomatic posturing that threatens the conference and welcoming the proposed inter-governmental UN body the founding of which will be discussed at Stockholm. At the same time they offered their own suggestions for submission to Stockholm.

Effectively what they propose is that

business, labour, scientists and consumers should organize bodies to manage the environment on a global scale, if necessary without the cooperation of governments which may have to be included later. The oceans, they say, are the obvious places to try this first. The meeting said that for such a body to be effective it must have a source of income independent of governments, and suggests a levy on ocean users as the answer. At the same time the meeting was careful to support the Stockholm conference, pointing out that the proposed environmental body will have an important, if toothless, role in initiating agreements on standards. What the conference would like to see is the new body eventually founding an International Environment Tribunal, along with a force to police pollution on the high seas and the like, and enough power to force national governments to fall into line on anti-pollution measures.

But the official statement of the conference lacked the bite of some of the background papers for the meeting in which, for example, Francis Gerard, editor of *Industries Atomiques*, suggested an outline structure for an environmental agency independent of, but affiliated to, the United Nations (as, for instance, is the International Atomic Energy Agency).

The statement to emerge from the meeting was nonetheless rather more positive than most committee-prepared documents, but it remains doubtful if it will have any effect on the course to be taken in Stockholm.



## CYANIDE

## Price of Safety

THE quantities of sodium cyanide waste dumped in the Midlands recently could have been disposed of for as little as £30 or £40. ICI, the largest manufacturer of inorganic cyanide in Britain, offer a disposal service to those companies buying cyanide from them. For a charge of about £2 a 2 cwt drum, they will collect and dispose of the cyanide waste that results, for example, from the case-hardening of steel. The quantities recently dumped in the Midlands have varied from 6 cwt to about 40 cwt in each instance.

Not that ICI's disposal methods would appeal to everybody. They seal and weight the containers whose contents are about 10 per cent cyanide and, with the approval of the Ministry of Agriculture, Fisheries and Food, drop them into 2,000 fathoms of water in mid-Atlantic. ICI dispose of 1,000 tons of cyanide waste in this way annually—business worth some £20,000 a year.

Other methods of cyanide disposal are more expensive. Cyanide waste comes either as a solid or a liquid. The solid comes from the casehardening of steel, where sodium cyanide is used in a heat treatment bath. During use the cyanide content of the bath decreases, and when the levels reach about 10 per cent the bath is drained and then refilled.

The liquid comes mostly from electroplating. The cyanide content is only about 5 per cent and is not consumed in the process, but the mix is discarded after a time because of contamination by metals. The larger electroplating companies almost all do their own cyanide disposal on site.

Both forms of the waste are comparatively easy to dispose of by chemical means, because cyanide is relatively unstable. The liquid waste is mixed with chlorine in one form or another and the result is a non-toxic fluid. The solid waste is treated in the same way after being turned into a solution.

The cost of chemical decomposition, however, is much higher than disposal at sea. A ton of solid waste costs about £100 to treat, and transport to one of the three companies with treatment plants in Britain would add substantially to that bill. The liquid waste disposal costs about £30 for 1,000 gallons, again exclusive of transport costs.

Cyanide can also be buried very effectively. Because it is unstable it decomposes in a matter of months if spread around a controlled site which is in a very active microbiological state and, provided the anaerobes which thrive on cyanide and other carbonates are not disturbed, the tip can be used repeatedly.

It is clear, however, that in spite of the current concern, cyanide is one of the lesser disposal problems that in-

dustry and society face. Waste metals, for example, provide a bigger disposal problem and the only simple solution to their disposal is to bury them. But with the introduction of stricter controls, both at sea (see *Nature*, 235, 414; 1972) and now on land in the form of the government's new bill, a higher price will have to be paid if toxic substances are to be disposed of safely.

It is, however, an ill wind which blows nobody any good. A spokesman for the Purle Group of waste disposal contractors said last week that following the cyanide scare their treatment plant is now working flat out and their incinerator, which three months ago was operating six days a month, is likewise at full stretch. "Heaven knows where it was all going before," he said.

## CERN

## Right on Schedule

THE construction of the 300 GeV accelerator in Geneva is on schedule, and physicists can still look forward to a 200 GeV beam of protons in 1976. Decisions to be taken at the end of 1973 will determine whether or not 300 GeV will be available for experiments in a new experimental hall by 1979.

The first annual report of the 300 GeV programme published recently (CERN/1050) shows, with all credit to Dr J. B. Adams, the director general of the project, that the costs of the project are still within the budget of SF 1,150 million, at 1970 prices, which was agreed a year ago, and it is gratifying that contracts awarded for the first two principal civil engineering projects did not exceed the estimated costs.

A year ago the project had four full time employees, but now there are 100 staff members with a further thirty appointments earmarked. There should be 415 staff members by the end of 1972, and by 1976 the number will have risen to 730. Between then and the end of the project in 1979 it is planned to employ only forty more people. The maximum complement of 770 will be made up of 500 people in the machine group and 270 in the services group.

The decision whether or not superconducting magnets are to be incorporated has to be taken at the end of 1973. This decision will determine whether 300 GeV protons will eventually be available or whether higher energies—possibly 1,000 GeV—will ultimately be attained.

The first stage of the machine, which will be completed by 1976, will have two magnets in each of 217 sectors of the accelerator. At most there will be room available for two more magnets in each sector. At the end of 1973 a decision will have to be taken on the type of magnet to put into the gaps.

This will be dictated by the budget, and also by the state of the technology of superconducting magnets at that time.

The necessarily stringent budget of the project, according to the report, implies that "any modifications or options arising in the course of the programme can only be adopted if they do not increase the costs of the programme". The budget allocates SF 516.3 million to the planned second stage—to increase the energy to 300 GeV by incorporating a third conventional magnet in each sector as well as to build a new experimental area. If it is thought scientifically worthwhile to obtain protons of energy higher than 300 GeV, and if, as seems probable, the cost of superconducting magnets turns out to be much more than the cost of conventional magnets, then in eighteen months' time a plea will have to be made to the contributing countries for more money.

Superconducting magnets incorporated in the second stage will raise the maximum energy to 700 GeV, of which 500 GeV will be obtained from the superconducting stage and the remainder from the first stage. If a decision is taken later to remove the first stage conventional magnets and replace them with superconducting magnets, then a maximum energy of 1,000 GeV will be obtainable.

There is now a definite commitment to build a 200 GeV accelerator, and it is only if "the physics interest makes the extra 100 GeV important" that a decision will be made to go ahead and add the extra magnets. At the end of 1973 results of experiments carried out with the 200 GeV machine at Batavia, Illinois, which went into operation last week, will be available, and these should help in coming to a decision about whether or not to increase the energy of the CERN machine.

The decision to be taken at the end of 1973 will also depend critically on the progress made with the superconducting accelerator under construction at Stanford University. Difficulties experienced at Stanford in the past twelve months have raised doubts about the future of superconducting accelerators, and perhaps the end of 1973 will come too soon for all the difficulties to have been ironed out. This could save the decision makers at CERN from having to go back to their respective governments cap in hand.

The difficulties experienced with obtaining the required voltage gradient on the accelerator sections at Stanford are resounding all through the nuclear physics world. Once these problems are ironed out, the application of superconducting technology to heavy particle accelerators—in contrast to electron accelerators—will become more than a pipe dream.

# Chemistry in Africa

This account of a conference held in Africa last December is by Professor F. R. Jevons, Department of Liberal Studies in Science, University of Manchester.

"CHEMISTRY is chemistry is chemistry," as Gertrude Stein might have said, had she been thinking of the supposedly supranational nature of science. But is science really so autonomous an activity as to be in essence the same in all countries of the world? It would be convenient if there were such a platonically ideal chemistry; it could then be taught in the same way everywhere, merely adding topical applications as adventitious appendages to give local colour. Or is there such a thing as African chemistry?

This question formed the most interesting theme running through the conference on University Chemistry Teaching held in the University of Nairobi, Kenya, December 14–18, 1971. The first such conference to be held in Africa, it was conceived and organized by D. McCormick of the Chemistry Department at Nairobi. About 70 delegates came from African countries, mostly the English-speaking ones south of the Sahara, but not including Rhodesia and South Africa. Africans, it was pleasing to note, were not too heavily outnumbered by expatriates.

## School Curriculum

Even at school level, science is no longer regarded as so international that Nuffield, Chemstudy and Chemical Bond Approach can be thought of as brand names for which to set up local dealerships. Since 1968 the East African Secondary Science Project has been adapting Nuffield materials. The dominant feeling now is that one cannot go faster than the teachers can assimilate new approaches and one wonders how far "curriculum development" is really in-service training in disguise. In Lusaka, in the Curriculum Development Centre of the Zambian Ministry of Education, J. V. Huxley is producing booklets describing local chemistry-based industry so as to help expatriate teachers add relevance in spite of their usually short periods of service in the country. Zambia, however, differs from the countries of the East African Community (Kenya, Tanzania and Uganda) in that there are no sixth forms in the schools and first degree courses last four instead of three years.

With a few exceptions, like select Strathmore College in Nairobi, which prefers the London Board, most East

African schools have been taking the Cambridge Board's examinations. Now, however, many scripts are being marked locally. Socialist Tanzania has fewer secondary schools than Kenya but the proportion of Africans among the teachers is higher. Most of the science students at the University of Dar-es-Salaam are bonded to enter school-teaching after they graduate.

## Objectives in Developing Countries

On the first day of the Nairobi conference there was a series of papers on objectives for chemistry in developing countries. Calls for university courses to be made relevant to Africa were strong and insistent; speaker after speaker rejected the too theoretical and esoteric aspects of "western" chemistry. F. G. Torto (Accra, Ghana), elder statesman among the African chemists, maintained that objectives should be related to the circumstances of the countries concerned and the need to advance their economic and social development.

Laboratory and workshop techniques should be taught to help graduates when they enter school or industrial laboratories poorly equipped with technicians. B. A. Dadson (Cape Coast, Ghana) felt that statistical thermodynamics and ligand field theory belong to "the dimly lit corridors of Oxford and Cambridge" and that African students should learn instead to understand their chemical environments and to see the potentialities of their own countries' resources.

## Relevance in Teaching

The trouble with the "chemistry is chemistry" view, according to G. M. Barrow (ex-Case-Western Reserve), is that it makes the subject into a dead corpus of knowledge which can be given relevance only by adding or subtracting examples to or from the mummified body. He spoke of making chemistry respond automatically to time and place and of allowing it to grow organically as a study of the world the student lives in.

Barrow's eloquence did not still all doubts. How parochial does relevance have to be? Examples can be given to students from the first year onwards, said S. R. Landor (Makerere, Uganda), but they can be internationally relevant. One of the ubiquitous doubts about relevance—whether teaching can be related to the world outside the labora-

tory and at the same time serve as an adequate preparation for further study—remained unanswered except by affirmation of faith. In any case, said J. A. Leisten, Barrow's approach is not needed for his students in Malawi. Unlike many of those in advanced countries, they are highly motivated and do not need to be coaxed into taking an interest.

As well as attempts to make science courses relevant in themselves, there are naturally courses of the "science and society" type which try to add relevance as an accessory to science. "African Studies" at the University of Zambia seems to have had a mixed success so far. At Dar-es-Salaam, there is a course in "Development Studies" for all students; the course outline mentions topics like self-reliance, Tanzania socialism and "the need for a scientific and socialist view of industry in nature".

## Practical Work

Many science teachers in Africa comment on the "anti-science" background of their students, but it should really be called non-science, because it is different from the sophisticated rejection of "scientific" values and modes of thought by western devotees of authors like H. Marcuse and T. Roszak. In such circumstances one would expect practical work to play a specially important role. Indeed, McCormick said, with characteristic vigour, "the lab's the place to hit them". Similarly, Joy Redhead (Ibadan, Nigeria) gave an enthusiastic exposition of the wide range of virtues of laboratory classes.

## African Unity

Throughout the discussions at Nairobi there was no doubt that the chief problems were felt to be common to all the African countries. D. Odhiambo (Nairobi) suggested establishing an African journal and before the end of the conference preliminary steps were taken to implement this idea. Perhaps the journal will be called *Chemistry in Africa*. Later it may be possible to bring in the French-speaking countries and make it bilingual. There is little chance, though, of using national languages. Although Swahili comes close to being a lingua franca in East Africa, other countries are less fortunate. Zambia alone is reckoned to have about 70 languages, of which seven have been selected as media for teaching in schools. In these circumstances, the real hope must lie with the material for English medium primary education which has also been prepared.

I thank the British Council for making it possible for me to visit Zambia, Kenya and Tanzania.



## NEW WORLD

# Second Crusade Begins to Move

by our Washington Correspondent

THIS year's health research crusade was quietly launched last week on Capitol Hill. Identical bills calling for expenditures of \$1,290 million over the next three years in a "national attack on diseases of the heart and blood vessels, lungs and blood" were introduced into the Senate by Edward M. Kennedy and into the House of Representatives by Paul G. Rogers. If enacted unchanged, the bills would almost double the present rate of expenditure on such diseases by 1976, and the money envisaged for 1973 alone is some \$115 million more than President Nixon requested in his budget message.

Those who were predicting last year that the National Heart and Lung Institute would not be far behind the National Cancer Institute in the competition for funds and public attention, therefore seem to have been close to the mark. Indeed, the arguments for increasing federal expenditure on diseases of the heart and lungs are perhaps more compelling and almost as politically appealing as those that were advanced last year for injecting more money into cancer research. For do they not account for more than half the deaths each year in the United States? And it will be doubly difficult for both Congress and the Administration to oppose such expenditure in an election year.

The arguments for increased efforts to combat diseases of the lungs and circulatory system cannot, however, be passed off merely as election year political rhetoric. Last year, for example, a committee of scientists appointed by Dr Theodore Cooper, Director of the National Heart and Lung Institute, recommended a greatly expanded federal programme to combat arteriosclerosis (see *Nature*, **234**, 435; 1971) on the basis that the very high incidence of the disease in the United States may be cut down by improved screening and educational services. Moreover, unlike cancer, many of the underlying causes of diseases of the lungs and blood vessels are relatively well understood, and investments are therefore more likely to bring greater success in terms of reducing the incidence of such diseases.

Mr Paul Rogers suggested last week, for example, that if his bill is enacted, the number of deaths from heart attacks and strokes might be reduced by up to 30 per cent in five to seven years. And Dr Charles K. Friedberg, a member of

the task force on arteriosclerosis, said, when that report was released, that if Americans were prepared to alter their dietary habits and to stop smoking, the incidence of arteriosclerosis in the United States may be reduced by as much as 90 per cent.

The chief provision in the Kennedy-Rogers bill is the expenditure that it calls for. The bill embodies few major changes in either the organization of the National Heart and Lung Institute or in the programmes that it administers. In short, what the bill calls for is the setting up of a national heart, blood vessel, lung and blood disease programme, on the lines of the cancer programme, embodying research into the epidemiology, aetiology and prevention of cardiovascular and lung diseases, the establishment of new diagnostic and treatment centres and educational programmes.

The programme would be reviewed by Congress each year, thereby establishing some measure of Congressional control over the National Heart and Lung Institute. There is also provision for the setting up of fifteen new clinical research and training centres, and the present National Heart Advisory Council would be replaced by an advisory board consisting of twenty-two members appointed by the Secretary of Health, Education and Welfare, on the lines of the newly created National Advisory Cancer Board. Also borrowed from the cancer legislation is the suggestion that all grants of less than \$35,000 could be approved by the director of the institute after peer review, but without going before the advisory board.

Mercifully, the bills do not require that research into heart and lung diseases should be taken out of the hands of the National Institutes of Health so that, unlike the past year's debate on cancer research, arguments about the merits of increasing expenditures on research and treatment will not become side-tracked by wrangles over important administrative arrangements. The National Heart and Lung Institute would, moreover, have less independence under the terms of this bill than the National Cancer Institute now has, for there is no suggestion that its budget should be separated from that of the rest of the National Institutes of Health. The expenditures being asked for are \$370 million in 1973, \$430 million in 1974 and \$480 million in 1975.

The importance of these two bills, compared with the rash of other legislation concerned with funding for research into heart and lung diseases, is that they have been introduced by the two most influential Congressmen in health matters—Rogers is chairman of the House of Representatives Subcommittee on Public Health and Environment, and Kennedy is chairman of the Health Subcommittee of the Senate Labor and Public Welfare Committee—and they have also been co-sponsored by a majority of members of the respective subcommittees. It is also a welcome sign that Kennedy and Rogers, who have in the past been at odds over the cancer legislation and over bills concerned with drug abuse control, have cooperated on this legislation.

The Administration has also declared its interest in supporting such research and treatment. President Nixon mentioned in his State of the Union message that he is soon to appoint a task force to see what needs to be done at the federal level in supporting such activities, and this commitment was reiterated in his health message to Congress last month. Officials of the National Heart and Lung Institute, although reserving their judgment on the Kennedy-Rogers bill, are understandably happy about the attention the institute is receiving. One official also pointed out last week that provision of extra funds, and especially some loosening of the present restrictions on hiring manpower, will help the institute to build up its own in-house research capabilities.

## OCEAN MAMMALS

### A Five Year Reprieve

by our Washington Correspondent

CENTURIES of unfettered killing, motivated by commercial greed, have driven one species of whale after another to the verge of extinction. And there is now mounting concern that other varieties of ocean mammals, such as some species of seals and polar bears, are following their marine relatives down the slippery slope to the point at which they can no longer survive. It is therefore no surprise that in the United States, where conservationists have a particularly strong voice and a hefty political punch, public anger has finally forced the federal government to take strong and urgent steps to remedy the situation.



In December last year, a total ban on whale fishing by US vessels and on the import of whale goods was brought into effect, and the House of Representatives defeated a bill seeking to protect other species of ocean mammals because it was too weak, and at the time there was no chance for Congressmen to amend it. Last week, however, they had their chance when the bill was brought up for a second time, and the chance was not missed. A provision calling for a five-year moratorium on the killing of all ocean mammals (albeit with a few loopholes) and on the import into the United States of all marine mammal products was tacked on to the bill before it was passed by a lopsided margin of 362 to 10. Similar legislation was also the subject of hearings before the Senate Commerce Committee.

In addition to declaring a moratorium on the killing of marine mammals, the bill sets up within the Commerce Department a three-man commission, advised by a scientific committee, to review the condition of stocks of marine mammals and to advise the Secretary of Commerce and the Congress on the workings of the act.

When the bill was first brought before the House of Representatives in December, it was essentially a measure calling for better management of marine resources, rather than an outright protection device. Its central theme, for example, was that the Secretary of Commerce would issue permits for the killing of marine mammals on a commercial basis, although permits would not be issued for endangered species, and the issue of any permit would be subject to public hearings. The bill, however, came in for a barrage of criticism from the press for being too weak, the most commonly heard criticism being that the Commerce Department is too susceptible to industrial pressure to provide sufficient guarantee of protection to marine mammals. With the history of the International Whaling Commission's patent inability to prevent the demise of the blue whale as ammunition, public opinion had sufficient fire power to shoot it down.

The bill that was passed last week, however, calls for a five-year moratorium on the killing of marine mammals before any such permits are issued, although there are a few important exceptions to the blanket ban. Permits would still be issued for taking marine mammals for scientific research or for zoological gardens provided that such action does not constitute too grave a threat to the species; there is no outright ban on the incidental capture of porpoises and dolphins during tuna fishing, provided that adequate precautions are taken to prevent these mammals from being trapped in the tuna nets; and

finally the bill allows continued capture of the North Pacific fur seal under the conditions of an existing treaty signed by the governments of the United States, Japan, Canada and the Soviet Union. Morris Udall of Arizona and David Pryor of Arkansas, chief sponsors of the amendment calling for the moratorium, both suggested that they would prefer a blanket ban, but that such a goal is not politically possible this year.

The moratorium, if it survives the rest of the Congressional mill, will have at least one potentially large effect on international negotiations for the preservation of marine mammals: United States representatives can now approach such negotiations free from vested commercial interests. As Rogers C. B. Morton, Secretary of the Interior, said when he announced the US moratorium on whale fishing: "We are in a position to provide leadership in the world wide drive to preserve the whale as a vital part of the marine ecosystem." That leadership can now be turned to other equally threatened species which are being killed either in the name of "sport" or for luxury products for which there are adequate substitutes.

## POLLUTION

### The Cleaning Bill

by our Washington Correspondent

WHILE the interminable debate about the merits of economic growth has been raging in the popular and scientific press, the Environmental Protection Agency and the Council on Environmental Quality have been looking into the likely effects of present pollution control requirements. They have found that, as expected, federal pollution abatement standards are likely to depress the growth in gross national product, increase prices and cause a few plants to shut down. But the overriding finding is that these effects will not be severe, and they will not result in unbearable costs in individual industries.

The EPA and CEQ studies, made public this week, were carried out by private consultants on the basis of assumptions and constraints supplied by the agencies. Fourteen studies were made on the likely economic impact of pollution control requirements on individual industries between 1972 and 1976, and one estimate was made of the effect of pollution control on the entire economy between 1972 and 1980. The assumptions, however, inevitably cast a few shadows of doubt on the findings. For example, no account was taken of the likely effects of the sulphur tax proposed recently by the Administra-

tion, and as far as water quality standards are concerned, the estimates were made on the basis that corporations would be forced to use "the best practicable" technology, rather than the more stringent requirement of "the best available" technology that is likely to be stipulated in the bills now before Congress.

Nevertheless, the estimates for the economy as a whole are that pollution standards will cost industry some \$26,000 million over the 1972-80 period, thereby pushing up prices, especially in the early part of the decade when installation costs are greatest, and decreasing the rate of growth of the gross national product by some 0.1 per cent over the entire period. Unemployment is also expected to be increased by between 0.1 and 0.2 per cent a year because of pollution control requirements. None of these effects is, however, likely to be too keenly felt, and they can mostly be offset by compensatory policies. But such policies, the report indicates, would tend to push up prices by some 3 per cent a year, and consequently affect adversely the trade balance.

The estimated effects on individual industries are, however, perhaps more revealing. The first thing to be said is that no industry will be severely affected, although profits will naturally decline for corporations which cannot pass on the full cost of pollution control. Four industries—paper, steel, petroleum and electric utilities—are expected to invest more than \$1,000 million each during the 1972-76 period, with electricity utilities the hardest hit with an investment of between \$8,000 and \$11,000 million.

Over all the industries studied, some 200 to 300 plants are expected to be forced to close by the additional costs of pollution control, but the reports make clear that "the vast majority of these were likely to have closed in the 1976-1980 period in any case". These closures are expected to result in the loss of between 50,000 and 125,000 jobs, which will substantially affect some 50 to 150 communities. The worst affected industries turn out to be fruit and vegetable canning and processing, paper and iron foundries.

One important point about the estimates is that they take no account of the likely benefits, both hidden and direct, of pollution control, and they make no attempt to calculate the effect of similar processes in other countries—a factor which will have a substantial effect on the balance of payments.

It is indeed appropriate that the Environmental Protection Agency and the Council on Environmental Quality have instigated these investigations at a time when the effect of pollution control on the gross national product is uppermost in people's minds.

## NEWS AND VIEWS

## Threat to the British Elm

SINCE the introduction of elm disease into Britain in the early years of this century, the pattern and impact of its development have been unique. The establishment of epidemic conditions in the south of England following soon after the first record of this disease in Hertfordshire in 1927, and its dramatic decline in the late nineteen-thirties, have been well described by Peace (*For. Comm. Bull.*, No. 33, 1960). Following this and until about 1967, the disease was apparently quiescent with widespread infection rarely causing more than slight damage to tree crowns. The development of epidemic conditions in several areas of the south of England and Wales has recently received much publicity, but the inadequate data available on the early stages of the present epidemic are perhaps symptomatic of a deficiency in the plant pathology services in Britain at present.

The isolation of an aggressive strain of the causal fungus, *Ceratocystis ulmi*, in Britain described by Gibbs, Heybroek and Holmes on page 121 of this issue of *Nature* is a significant and serious development, particularly in view of the demonstration of its virulence on Dutch clones of proven moderate and high resistance to other strains of the pathogen. Although ominous, this demonstration of increased virulence of the pathogen at least seems to provide some concrete explanation, albeit perhaps partial, for the development of the present severity of the disease in Britain.

The disease in its present severe form has a special importance in several ways. Over large areas of southern England, elms are a major and often the principal tree component in the rural landscape. The erosion of tree populations in non-forest conditions in Britain is progressing at an alarming rate, as a result of many factors quite unrelated to disease and principally as a consequence, directly or indirectly, of mechanization and economic developments in agriculture. cursory examination of the landscape from the motor car or train gives rise to little cause for immediate pessimism, except in certain areas, but even the most superficial assessment of the age/class distribution of trees in hedgerows reveals a serious situation which will undoubtedly become only too evident generally in the next fifty years or so. The Forestry Commission's *Census of Woodlands 1965-67* (1970) records a reduction in the percentage of saplings in the general population of hedgerow trees in central and southern England from 31 to 23 per cent in the period between 1951 and 1965, and indicates that this decline will continue in the future. In this context, the rehabilitation of elm populations in the present seriously diseased areas is likely to be less successful than after the epidemic in the 1930s. Elm has a special social and structural significance in some of the new towns and other new development areas in Kent and Essex, counties which are both severely affected by elm disease, and the high mortality of street, garden and park trees in these areas will have a very serious and lasting effect on general amenity.

The effect of elm disease on the home-grown timber industry is uncertain. The geographic pattern of saw-

log supply is likely to be much affected, but unless the disease becomes established at an epidemic level over much larger areas of the country, the effect on overall availability of home-grown elm timber is likely to be small. At present and in the immediate future, the glut in supply arising from control felling of infected trees, and other difficulties imposed by the requirements of the elm disease orders, have created and will continue to produce anomalous market conditions. There is, of course, concern for the effects of the disease on general ecological patterns of plant and animal life, but although these effects will undoubtedly be profound, assessment of these influences will be difficult until the long-term or permanent effect on tree populations becomes clear.

Ecologically, the decline of the elm on any considerable scale in Britain, whether or not caused by disease, is likely to affect the rate of deterioration of the human environment directly and demonstrably. The same is true for several other tree species common both in rural and urban areas, notably oak, beech, sycamore, ash, lime, horsechestnut and birch. Some of these trees, particularly oak and beech, have considerable economic importance in timber production, but the amenity and recreative value of all these species, and of many other species of smaller trees with more restricted urban distribution, is incalculable.

The publicity and genuine public concern generated by the serious mortality of elms caused by elm disease have in many ways been remarkable. Unfortunately, this interest has been almost entirely confined to elm, and has done little to stress the potentially delicate position of several other amenity tree species already and potentially subject to disease and pest infestations, or threatened by other drastically adverse factors of the environment. Few people outside the ranks of forest pathologists even know of the epidemic establishment of killing infection by the fungus *Cryptostroma corticale* on sycamore in north-east London in the late 1940s, which declined as dramatically as it arose, but left the fungus established in London and elsewhere. A general decline of ash resulting from unknown factors is prevalent throughout Britain, but no investigation into its extent and cause seems to have been initiated. Amenity species, including those with some considerable economic value, fall into the research void present between the activities of the various research establishments concerned with horticultural crops (although, even here, tree nursery problems receive comparatively little attention) on the one hand, and the work of the Forestry Commission's Research Division with its primary (but not exclusive) concern with forest tree species, on the other.

This dearth of research activity on amenity trees applies to a wide range of physiological and cultural, as well as pathological, problems. The value of amenity tree nursery stock in Britain, estimated recently to exceed a hundred million pounds, and the large sums spent by public and other authorities on the establishment and care of larger trees surely warrant the development of commensurate research and extension services.—R. G. P.



# How Australia used Cannikin

WHATEVER one's views on the advisability of firing the five megaton explosion Cannikin on Amchitka, one cannot but acknowledge that the seismologist was handed a remarkable new source on a plate. In today's *Nature* (page 111), Cleary, Simpson and Muirhead describe an experiment of some importance that they performed with Cannikin.

It has been known for some time that the seismic velocity/depth curve within the Earth has some regional variations. Effects arising from underthrust slabs are well known and fairly local in extent, but there are broader regional trends which can only be as well documented as the distribution of seismic stations permits. A major contribution to understanding of the seismic velocity variation necessarily comes from travel time residuals (these are differences between observed arrival times of P-waves at seismometers and the theoretical arrival time from a standard set of tables). It is found that residuals vary from about  $-1.5$  to  $+1.5$  s (a negative residual implies a signal arriving earlier than predicted). There is a fairly good correlation between negative residuals and stable, very old continental regions called shields. Positive residuals tend to occur in regions of recent tectonic activity.

Information on residuals has usually had to be extracted from records of hundreds of earthquakes, and has had to be carefully sifted to ensure that all other sources of bias have been eliminated. Cleary *et al.*, however, have been able to circumvent this detailed analysis by using a source at an exactly known time and place. Just before Cannikin was fired they installed a set of temporary stations across about half of Australia. Arrival times at each station were measured and converted into residuals which are taken to be indicative of seismic velocity variation across the continent. They assume (with considerable justification) that near-source heterogeneity will not affect in any differential way the suite of rays departing steeply and southerly towards the network. They assume, also reasonably, that the deep part of each ray path is probably almost identical. Furthermore, by placing all the seismometers at almost the same distance from Cannikin they have eliminated any errors arising from inaccurate reference time tables. Thus they can conclude that the variation in residual of from  $-1.0$  s to  $+0.5$  s is most likely to be indicative of lateral variations in seismic velocity in the upper mantle underneath Australia. The variation is such that seismic velocities are higher in the west.

It is impossible from one number at each station to give anything more detailed than an integrated view of the seismic velocity profile. Nevertheless the integrated contrast is very striking and significant. If it were spread out over, say, the top 600 km it would correspond to a 2.5 per cent seismic velocity contrast. Confined to a narrower region the contrast, of course, goes up. Cleary *et al.* suggest that the lateral variation may be related to the disappearance of a low velocity layer. They are particularly impressed by a rapid velocity change over about 500 km in the centre of the profile and this region includes the eastern boundary of Precambrian rocks. It is thus geophysically plausible that the low velocity layer disappears here. If so, the contrast, presumed confined

dominantly to the depths from 100 km to 300 km, would be about 6 per cent.

From this well controlled experiment has emerged a good first step towards understanding the structure deep beneath the Australian continent. The results are of importance, however, to the geophysics of continental regions in general.—D. D.

## Viruses and Breast Cancer

A LITTLE more than a year ago Moore and Charney and their colleagues caused great excitement with their report that certain human milks contain particles which have a structure virtually identical to that of mouse mammary tumour virus. They wrote (*Nature*, **229**, 611; 1971), "We conclude that the similarities between adenocarcinoma of the breast in mice and women are too extensive to be coincidental and that human breast cancer may also be a viral disease." Since that was written, Moore's group in collaboration with Spiegelman's group have shown that there are particles in human milk which contain reverse transcriptase and a 70S single stranded RNA. Moreover, Spiegelman and his colleagues (*Nature*, **235**, 32; 1972) have, by nucleic acid hybridization experiments, shown that human cancerous breast cells but not healthy breast cells contain RNA molecules which are homologous to those of mouse mammary tumour virus RNA. Such findings, of course, have done much to strengthen the argument that an RNA tumour virus might be involved in the aetiology of human breast cancer, perhaps acting as a necessary, if not sufficient, aetiological agent. It is ironic, therefore, to read on page 103 of this issue of *Nature* that Moore and his colleagues have lately been having second thoughts about the various particles to be found in human milk and are now considerably more cautious about what significance might be attached to them.

Moore and his colleagues have done two things: first, they have now screened for viruses and other particulate matter 381 samples of milk from 263 American women, ninety of whom are from families with a history of breast cancer; second, they have paid greater attention to the structure, as revealed in the electron microscope, of the various milk particles, and have introduced a new nomenclature for these particles, which they believe will help avoid confusion although others may well argue that it is more likely to confound confusion.

In their earlier work and reports Moore *et al.* apparently classified any virus-like particle in human milk, which had regular spikes projecting from its surface, as a B-type particle and therefore similar to mouse mammary tumour virus. Now, however, they say that very few human milks (thirteen out of the total sample) contain particles which very closely resemble B-type particles of mouse mammary tumour virus and these particles in human milk should be called MS-1 particles. A larger proportion of the human milks contain spiked particles (MS-2 particles) not identical to the mouse virus but which may be partially degraded forms of MS-1 particles.

Finally, the virus-like particles most commonly found in human milks bear no resemblance to mouse mammary tumour virus B-type particles but do resemble mouse leukaemia virus particles. In short, it seems that no great significance can be attached to the structure of the various virus-like particles which can be isolated from human milks.

But what about the comparative distribution of these various virus-like particles in the milks of women from families with or without a history of breast cancer? Last year Moore *et al.* reported that B-type particles (structures which they would now classify either as MS-1 or MS-2 particles) were present in the milk of six out of ten American women belonging to families with a history of the disease, but only seven of the milks from 156 women belonging to families without a history of the disease contained particles. Moreover, they also reported that B-type particles were present in eighteen of the milks from forty-six Parsi women. There seemed to be a strong positive correlation between the occurrence of the B-type particles and an increased risk to an incidence of breast cancer but, now a larger number of milks has been screened, that correlation no longer applies. The data given by Moore *et al.* in their first table (page 105) indicate that there is no significant difference between the distribution of any of the three types of virus-like particles which they identify in milks from women belonging to families with or without a history of the disease.

One has only to read the last paragraph of what Moore *et al.* have to say in this issue of *Nature* and then immediately re-read the last paragraph of their report published last year to realize the extent to which doubts and uncertainties have replaced confident enthusiasm about the role of putative viruses in human breast cancer.—From our Cell Biology Correspondent.

#### CYCLIC AMP

### Role in Higher Plants

from our Plant Physiology Correspondent

THE action of cyclic 3',5'-adenosine monophosphate (cyclic AMP) as the mediator of the action of several mammalian hormones has been established for some years. Similarly, its activity as an important intermediary in the regulation of gene expression in certain microorganisms is now recognized. Consequently, observers with a feeling for the unity of biochemistry will doubtless have been relieved by the recent flush of articles reporting a possible role for cyclic AMP in the third

major group in the biosphere, the green plants.

Latest among these reports is the finding by Henry Wood, Michael Lin and Armin Braun, at Rockefeller University, that a cell division factor, isolated from crown gall tumour tissues and chemically, though apparently not physiologically, closely related to the cytokinin hormones of healthy plants, is a potent inhibitor of cyclic AMP phosphodiesterase (*Proc. US Nat. Acad. Sci.*, **69**, 403; 1972). This enzyme catalyses the conversion of cyclic AMP to 5'-AMP and thus could operate to regulate the active levels of cyclic AMP within the cells. If cyclic AMP is postulated to be essential for cell division, the promotion of cell division by the extracted factor can be easily understood. The cell division factor is equally effective on bovine brain phosphodiesterase and on enzyme prepared from the crown gall tissues. Furthermore, Wood *et al.* report that adenylyl cyclase, an enzyme which catalyses the formation of cyclic AMP from ATP, can be detected as a membrane-bound component in the tumour tissues.

The first indication that cyclic AMP may have a role in hormone action in plants came from the finding that the nucleotide could replace the hormone gibberellic acid in the induction of  $\alpha$ -amylase synthesis in barley aleurone layers, tissues that store starch in the seed (Duffus and Duffus, *Experientia*,

**25**, 581; 1969; Galsky and Lippincott, *Pl. Cell Physiol.*, **10**, 607; 1969). Pollard (*Biochim. Biophys. Acta*, **201**, 511; 1970) was the first to obtain evidence for the formation of cyclic AMP in higher plants when he demonstrated the conversion of  $^{14}\text{C}$ -adenine into cyclic AMP by isolated aleurone layers. The rate of formation of cyclic AMP was doubled in aleurone layers which had been treated with gibberellic acid. Last year, using a similar approach, Saloman and Mascarenhas showed that auxin treatment of oat coleoptiles caused a 50–80 per cent increase in formation of cyclic AMP from exogenous adenine (*Life Sci.*, **10**, 879; 1971). Thus, although much remains to be done on, for example, the questions of specificity of action, and the magnitude and rapidity of the responses, evidence seems to be accumulating that cyclic AMP may be acting in plants, as in mammals, as an intracellular second messenger for auxins, gibberellins, and at least close relatives of the cytokinins.

From the point of view of the plant physiologist, of course, this is an exciting new development and it is likely that many related investigations have already begun in other laboratories in response to these findings. Nevertheless, it is somewhat saddening that the study of cyclic AMP in higher plants should have lagged so many years behind the exciting discoveries in microorganisms and higher animals.

### New Light on Folded DNA

NOT long ago Lerman reported that linear double-helical DNA could undergo a transition to a compact non-symmetric form in solutions containing neutral or acidic polymers and relatively high salt concentrations. Jordan, Lerman and Venable in next Wednesday's *Nature New Biology* (March 22) present a further investigation of this compact form (or  $\Psi$ -DNA, as they term it) by circular dichroism.

The principal conclusion is that the  $\Psi$ -DNA is characterized by a higher level of order, and not by any change in the geometry of the Watson-Crick helix. The formation of the  $\Psi$ -form is a process requiring some hours, and is accompanied by a gross change in the circular dichroism, the usual positive extremum in the long-wavelength region of the absorption band being completely eliminated. The appearance of this new form follows first-order kinetics. In due course the circular dichroism undergoes a further change, with a large enhancement in negative ellipticity ( $\Psi^*$ -form). The magnitude of the effect reaches a limiting value with neutral polymer (polyethylene oxide) concentration and with ionic strength. In the initial formation of the  $\Psi$ -form,

there is a sharp extremum in the profile of ellipticity against polymer and against salt concentration. This is evidently a kinetic effect only.

Jordan *et al.* explain the phenomenon in terms of the formation of nuclei for the growth of the ordered compact form: the conditions best favouring nucleation being different from those which correspond to maximum stability of the completed structure. The most likely mechanism involves the initial formation of an extended form of the DNA, which is then able to enter the new ordered compact state. X-ray diffraction indicates that the primary helix remains in the B-form.

The circular dichroism spectra resemble those of DNA in various forms, such as complexes with basic polymers, in which it is also presumed to be in a compact, condensed state. The form of the circular dichroism spectra is probably governed by inter-helix interactions. The DNA evidently does not interact directly with the neutral polymer, nor is the concentration of the latter sufficient to effect a large change in water activity. The thermodynamic reasons for the formation of the  $\Psi$ -form thus remain obscure.



## CHEMISTRY

## How does Na Corrode?

THE effect of liquid sodium on other metals has been a particularly happy hunting ground for chemists ever since it became clear that fast breeder nuclear reactors would make extensive use of sodium in their cooling systems. In the case of the prototype fast reactor (PFR) now being built at Dounreay in the north of Scotland, for example, it is not only the stainless steel pipes and the reactor vessel through which the sodium flows which have to be resistant to corrosion, but also the vital tantalum control rods.

It has been known for some time that oxygen dissolved in the sodium is instrumental in producing the corrosive effects. At 550° C in the presence of sodium flowing at 6 cm s<sup>-1</sup> — a rate comparable with the flow in the core of the PFR—the monthly rate of formation of tantalum corrosion products is 0.9 mg cm<sup>-2</sup> when the oxygen content is 10 p.p.m. and 3.8 mg cm<sup>-2</sup> when it is 40 p.p.m. Needless to say, the designers of sodium cooling systems for reactors have long been aware of the problems associated with dissolved oxygen and take careful steps to ensure that it is kept as low as possible.

In the first issue of the new-style *Journal of the Chemical Society, Dalton Transactions* (No. 1, 13; 1972), C. C. Addison, M. G. Barker and D. J. Wood, of the University of Nottingham, report experiments with which they have determined the precise chemical nature of the corrosion products of tantalum and, for good measure, niobium too. Although this is not to say that others, including Barker himself, have not had quite a clear idea of the nature of the tantalum corrosion product — namely mixed oxides of the general form Na<sub>3</sub>Mo<sub>4</sub>—this is the first time that an absolute identification has been made. One of the problems has been the difficulty of removing sodium from the surface of a tantalum or niobium sample without seriously disturbing the very corrosion products themselves.

Addison and his colleagues carried out an X-ray diffraction analysis of samples of tantalum and niobium, complete with their surface covering of sodium, after they had been immersed in static liquid sodium for several days and at several temperatures. Addison used both wire and plate samples, but in both cases the sodium film had to be protected against the formation by the atmosphere of sodium oxide. Wire samples were protected by a polystyrene film during the process of transfer to the X-ray powder camera, and the plates were examined in the (modified) sample chamber of the powder camera while dry nitrogen was passed through. After

the sample had been removed from the liquid, the corrosion product and the sodium layer were removed by scraping and by washing with water so that the lattice parameter of the tantalum or niobium metal near the surface could be accurately assessed.

The changes in the lattice constant of the metal, as compared with that of the pure metal, revealed increases indicative of the formation of a solid metal-oxygen solution in the first instance. At oxygen concentrations in the metal at 600° C greater than about 0.57 weight per cent for niobium and 0.21 weight per cent for tantalum, however, the Nottingham group say that Na<sub>3</sub>NbO<sub>4</sub> and Na<sub>3</sub>TaO<sub>4</sub> begin to form at the expense of the oxygen in solid solution with the metal.

## VIRUSES

## Finding Virus Proteins

from our Cell Biology Correspondent

THE chief obstacles to elucidating the replicative cycles of animal viruses stem not so much from the complexity of the virus particles themselves, many of which are less rather than more sophisticated than the bacteriophages, but from the complexity of their hosts, eukaryotic cells. The genomes of the small DNA tumour viruses, polyoma virus and simian virus 40, for example, have about the same coding capacity as the genome of phage  $\phi$ X174; the viral DNA can code for only about 200,000 daltons of polypeptide which means there are probably fewer than ten virus-specified proteins. Yet identifying any of these proteins, has proved and continues to prove extremely difficult because the synthesis of host cell protein continues unabated, indeed is probably en-

hanced, after infection. The problem is picking out a small number of proteins present only in extracts of infected cells from a large number of proteins present in both uninfected and infected cell extracts.

It seems generally agreed that polyoma virus and SV40 particles contain six species of polypeptide. The three minor and basic polypeptide species in polyoma virus particles, and presumably in SV40 particles, are probably derived from cellular histones as Frearson and Crawford (*J. Gen. Virol.*, **14**, 141; 1972) have recently concluded. On the other hand the major capsid protein must presumably be specified by the viral genome and, using double labelling procedures combined with SDS gel electrophoresis, Fischer and Sauer (*J. Virol.*, **9**, 1; 1972) have detected the major capsid protein and three other SV40 virion proteins in extracts of infected CV1 cells. These four proteins of the virion normally become detectable after the infection has reached the stage of viral DNA replication and, if DNA replication is blocked with cytosine arabinoside, the synthesis of detectable amounts of these four virion proteins is blocked. In such conditions, however, two virus induced, if not virus coded, proteins can be detected in cell extracts. Whether either of these proteins is SV40 T antigen of induced transplantation antigen remains to be seen, but as Fischer and Sauer modestly comment the identification in extracts of four virion and two non-virion but virus induced proteins is a first step towards the characterization of the proteins essential for the replication of SV40.

Ozer and Tegtmeyer (*ibid.*, 52) have also been investigating the synthesis of the major capsid protein of SV40 in AGMK cells and its assembly into

## Filamentous Fungal Viruses infect Yeasts

BACTERIOPHAGES characteristically are extremely host specific. By contrast many viruses of higher plants are able to infect species of more than one genus. Where do the double stranded RNA viruses recently isolated from such filamentous fungi as *Aspergillus niger* and *Penicillium stoloniferum* fit into this spectrum; are they as host specific as the bacteriophages or do they have a wide host range? As Lhoas reports in next Wednesday's *Nature New Biology* (March 22) it seems that in this respect the fungal viruses more closely resemble higher plant viruses than bacteriophages, for they can infect species of more than one genus.

Lhoas succeeded in infecting cells of the yeast *Saccharomyces cerevisiae* with virus isolated from *Aspergillus* and *Penicillium* sp. by exposing cultures of

mating yeasts to the virus. From the pattern of appearance of infected colonies of the yeast cells it seems that they are only infected when they are mating, perhaps because mating causes the breakdown of the yeast cell wall. The fact that yeast cells can be infected by these viruses strengthens the case for the conventional classification of yeasts as *Ascomycetes*.

As Border reports in an accompanying letter, under the electron microscope the development and structure of replicative forms of the virus particles in yeast cells can be seen to be very similar to those that develop in the natural hosts of these viruses, filamentous fungi. In short, these two sets of observations establish beyond doubt that at least some fungal viruses lack strict host specificity.

virions and empty capsids. They have used double labelling and gel electrophoresis and also immunological methods to identify capsid material. They seem to have raised antisera which permit the discrimination of virus particles and capsids from 8S capsid subunits and unassembled capsid protein molecules and with these they have shown, first, that throughout infection there is only a small pool of unassembled major capsid protein and, second, that empty but assembled capsids may well be precursors of complete, intact virions; during pulse chase experiments using  $^3\text{H}$ - or  $^{14}\text{C}$ -lysine to label capsid protein label incorporated during a 15 min to 2 h pulse into empty shells can subsequently be chased into virions.

On the other hand, Ozer (*ibid.*, 41), who developed the useful density cushion centrifugation procedure which allows the separation and therefore assay of virions and empty capsids, has obtained evidence which indicates that some of the empty shells that accumulate in infected cells arise from the breakdown of intact virions. Most virions, however, must assemble directly, for not only do the empty shells seem to be precursors of intact virions but when cytosine arabinoside is added late in infection the rate of virion formation is halved but empty shells continue to accumulate at an unabated rate. It seems, therefore, that the maturation of polyoma virus particles, like the maturation of polio virus, involves the assembly of an empty capsid which is then filled with viral nucleic acid.

## CANCER RESEARCH

### Kinetic Studies

from a Correspondent

At the eighth meeting in the series on the principles of experimental and clinical oncology at the British Museum (Natural History) on February 28, the topic for discussion was the growth rate and kinetics of tumours. The speakers were Dr G. G. Steel (Institute of Cancer Research)—an experimentalist inclined to mathematical models—and Professor L. G. Lajtha (Paterson Laboratories, Christie Hospital and Holt Radium Institute, Manchester)—a clinician similarly orientated.

Introducing his viewpoint Dr Steel promised to bear with the organizers and attempt to view the past with an eye on the future. Saddled with this intellectual divergence he first admitted that the lot of the kineticist studying tumours is not a happy one. Tumours are often heterogeneous in architecture, degree of necrosis and chromosomal phenotype and it is thus difficult to make a meaningful statement about such parameters as labelling index (with, for example, tritiated thymidine),

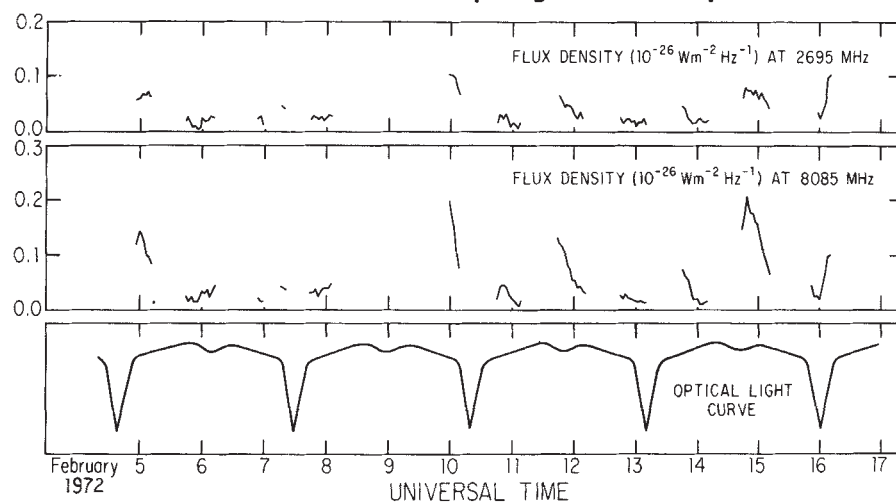
cell cycle time and growth fraction. Aside from this technical difficulty it is also very likely that the stem cells of a tumour, from which regeneration occurs after treatment, and upon which the growth rate of a tumour is eventually dependent, are not estimated by the averaged measurements easily available to the kineticist. Ascites tumours with high replicability are, felt Dr Steel, toys for the experimentalist which, however neat and tidy in their kinetics, provide relatively little information for the clinician. He compared the doubling times of various commonly used transplanted animal tumours with those found in patients in the clinic and showed that the animal tumours grow much faster.

Developing this theme Dr Steel went on to show that some human tumours grow very slowly indeed and that labelling studies often indicate a high rate of cell loss from the tumour mass. In experimental tumours it has been possible to correlate high labelling indices with high curability but in the clinic Dr Steel said that such a correlation has not been proved. He concluded that mathematical models of tumour growth have so far not contributed much to knowledge of human tumours and he felt that in the future

much more emphasis will have to be placed on characterization of the stem cell components of tumours.

Professor Lajtha defined kinetics as the movement of cells in time, number or functional capacity. Turning to leukaemia, the theme of his presentation, he put up a rough model of the three stem cell compartments in the haematopoietic system. The pluripotent stem cells are not recognizable, neither are the committed precursor cells. Recognizable precursors such as promyelocytes are the third category. Tumours can arise from any of the precursor types. It is difficult, however, to establish from which compartment a particular population of leukaemic cells was derived. In chronic myelogenous leukaemia, for example, it seems that there is a slight overproduction of myeloid cells and that they are released into the periphery earlier than normal. In such cases the Ph' chromosome is an indication of genetic abnormality, but other cells which apparently behave normally seem to possess the same defect. In some acute leukaemias the abnormal cells at first seem to have a slow rate of synthesis and a very low labelling index compared with such normal cell populations as promyeloblasts.

## Variations in Radio Stars $\beta$ Lyrae and $\beta$ Persei



ALTHOUGH only four radio stars with "normal" optical counterparts are known, these systems provide an important insight into astrophysical processes (see *Nature*, 235, 247; 1972) and are now the subject of intensive study by several groups of radio astronomers. In next Monday's *Nature Physical Science* (March 20) R. M. Hjellming, C. M. Wade and E. Webster report observations of both  $\beta$  Lyrae and  $\beta$  Persei. Both systems have changed dramatically since their discovery (Wade and Hjellming, *Nature*, 235, 270; 1972),  $\beta$  Lyrae having faded below the level of detectability and

$\beta$  Persei now showing irregular variations and flaring (see figure). There is no evidence of a correlation between this variability and the optical variations of the system. One of the radio flares—that of January 21–22 this year—has also been studied by V. A. Hughes and A. Woodsworth, and their observations are also reported in *Nature Physical Science* next week. The combined observations by the two teams cover 2.8 cm, 3.7 cm (8085 MHz) and 11.1 cm (2695 MHz) and represent the first observations of the decay of a radio flare of this kind at these frequencies.



## COSMOLOGY

**Conformally Invariant Theory of Gravity**

from a Correspondent

MOUNTING evidence against the simple steady state theory seems to have caused its last defenders, Hoyle and Narlikar, to abandon it. But their taste for controversy seems to be undiminished, and in two recent articles (*Mon. Not. Roy. Astron. Soc.*, **155**, 305 and 323; 1972) they show how to express the familiar Friedmann models in terms of their conformally invariant gravitational theory. This modification of the conventional approach has the effect of removing the big-bang from the models; in addition, Hoyle and Narlikar propose a new cosmological model.

The conformally invariant gravitational theory on which Hoyle and Narlikar's work is based was constructed in 1964 and is a generalization of general relativity. It is one of a class of theories into which the classical concept of a field—exemplified by the electromagnetic and gravitational fields—is not allowed to enter. The only variables admitted into theories of this type represent positions of interacting particles so that there is an economy of hypotheses. Supporters of direct particle theories, as they are called, claim also that it is illogical to have field theories which in principle admit source-free fields in view of the fact that reality is never ascribed to such fields. When the black-body background was discovered, for example, some cosmologists sought its origin in the creation of the universe whereas others looked for explanations in very distant objects; nobody took the background radiation to be a source-free field. This exhibits the redundancy of a field theory such as electromagnetism in which charged particles are thought of as consisting of nothing more than singularities which happen to exist in the fields, as against direct-particle theories in which the particles are fundamental and the field a mere mathematical device for assisting in calculating their mutual interaction. Direct particle theories can be constructed to correspond to all field-theoretic interactions. Frequently the change is largely formal; but the non-linear nature of general relativity gives rise to a greater difficulty of computing the corresponding direct particle interaction and to a theory which coincides with general relativity only when the number of particles involved is large.

This is an attractive feature of direct particle gravitation. It is in line with the ideas of Mach, who was struck by the fact that inertial frames in which Newtonian mechanics works, such as the Earth's surface (approximately), move

uniformly or are at rest relative to the distant stars. He sought to ascribe the phenomenon of inertia to the influence of distant matter. Einstein's general relativity can be thought of as an unsuccessful attempt to formulate a theory of inertia and gravitation in which the coincidence between inertial frames and the distant stars (or better the distant galaxies) is inevitable. Hoyle and Narlikar's direct particle gravitation succeeds here, for it is only the large number of particles whose interaction is considered, including distant galaxies, which makes local inertial and gravitational properties take their familiar form.

The phrase "conformally invariant" in the title of the theory refers to the fact that the form of the theory is unchanged, not only when the co-ordinate system is changed so that the frames are not necessarily at rest (as is allowed by general relativity) but also when scale factors analogous to changes of units are introduced into the geometry of the world, and these scale factors are allowed to vary with position and time.

Advances in understanding often reveal the most cherished beliefs to be mere conventions. For example, the invariance under non-constant scale factors admits the possibility that the trenchant arguments which cosmologists sometimes use to emphasize the puniness of the solar system and the short time scale of evolution may not apply in our universe. Some conformal frames are of course far more convenient than others. Hoyle and Narlikar show that the conventional frame is that in which all protons have the same mass and in which general relativity is a good approximation to their theory.

All direct particle theories are time symmetric, because interaction takes place between particles separated by time as well as space. This seems to disagree with the experimental observation that retarded interactions maintain causality and that waves emanate from sources in an unsymmetrical fashion. Direct particle theories can nonetheless be reconciled with causality provided that the absorption properties of the universe satisfy certain conditions. Until the new developments the only cosmology which satisfied these requirements (among the models usually considered) was the steady state theory. A belief in direct particle interaction was Hoyle and Narlikar's published reason for continuing to work with the steady state cosmology in spite of the growing evidence against it from source counts.

The first of their two new articles develops these ideas and shows how the conventional big-bang models can be made to appear in a different conformal frame. They show that the big-bang is an artefact of the conventional theory which (if conformal transformation is valid) can be made to disappear. One effect of this is to allow conformal invariance to be tested experimentally. If the homogeneity and isotropy of the black-body background radiation are accepted as incontrovertible evidence of its *primaeval* origin, the big-bang will have to be accepted as a physical occurrence and conformal invariance will have to be abandoned.

Hoyle and Narlikar naturally follow a different plan. They reject the conventional cosmologies as models of the universe, although they are admissible as pictures of what goes on on a smaller scale, and develop a new cosmology altogether. They point out that a new model is needed because of the failure of conventional cosmologies to explain the tendency of matter to clump together in a large scale in galaxies. Moreover, they maintain that the failure of conventional non-steady state cosmologies to meet the absorber requirements, referred to earlier, is a fatal objection. This point is extremely contentious.

In their second article Hoyle and Narlikar point out some features which would be desirable in a new cosmological model and show how they are allowed by conformally invariant gravitational theory coupled with continual creation. In order to get the absorber properties which they find desirable, while allowing the evolution of the universe for which there is such a wealth of evidence, they show how to revive an idea which was discussed in the 1930s by Dirac and by Eddington. This idea relates the strength of the gravitational interaction constant to the number of particles in the observable universe. This number decreases as the expansion takes particles over the limits of interaction imposed by the finite velocity of light faster than new ones are created. Thus the strength of gravitation diminishes. This is of interest to geophysicists for it would allow the resolution of some of the difficulties of understanding the drift of continents as an effect of diminishing gravitational cohesive force.

It will be of interest to see how the details of the proposed new model work out, and whether they follow in detail from a complete gravitational theory which includes a mechanism for the continual creation. But it is already of great interest to record that even the recent modified forms of the steady state cosmology seem to have been completely and formally abandoned by their two foremost protagonists.

# Making Conferences Successful

JOHN WRIGHT

Directorate of Overseas Surveys, Tolworth, Surbiton, Surrey

**Many features of the four-yearly Commonwealth Survey Officers' Conference contribute to its success and could be adopted with advantage by others.**

WE were very interested at the Directorate of Overseas Surveys (now part of the Foreign and Commonwealth Office's Overseas Development Administration) to read your correspondent's amusing remarks on "Symposia in General" (*Nature*, 230, 501; 1971). Since the Second World War we have been closely concerned with a conference for surveyors from all over the British Commonwealth which is held at four-yearly intervals; most of the procedures—and some others too—recommended by your correspondent indirectly (by overstating the opposite) have evolved at these conferences during the past twenty years or so. Moreover, at a recent United Nations Cartographic Conference in Tehran a number of measures, like limiting speakers' time, were introduced and a resolution proposing somewhat similar procedures for submitting papers was proposed and carried unanimously<sup>1</sup>.

The Commonwealth Survey Officers' Conference (to give it its full title) has three advantages over many international and even some other Commonwealth conferences. The first is that only one language is used, which cuts out microphones and interpreters. The second is that there has been continuity of organization, in that the conference has so far always been held in Britain and has not been peripatetic with all that this involves for a new team taking the task on every time and having to learn all the lessons afresh. The third is that because no registration fee is charged there can be some selection in the invitations and the size of the conference can be kept down—again cutting out microphones. The Greeks had a point when they insisted on limiting their assemblies to those which could be addressed by one man.

Because the most important members—those from Commonwealth departments—are normally the directors or assistant directors, they are interested in all the subjects discussed, and so there is never any splitting up into specialized commissions which is bad for any conference and fatal to a small one. Not only does it prevent something very real—the growth of the conference's own identity (and every body of people develops this) but it starves each specialized session of most of those gifted people in any conference who are able to make a contribution on any branch of the subject. There are usually only a score or so of these and they tend to carry the bulk of all the discussion between them. Split them up and the discussions are diluted.

It is worth mentioning that most of the basic procedures—now being appreciated increasingly by other conference organizers—were laid down by our first director, Brigadier Martin Hotine, who organized from here all those held after the war and who was himself president of those held in 1955, 1959 and 1963. The standing committee of the conference is formed by the heads of the four government survey departments—that is, Ordnance Survey, Hydrographic Department, Directorate of Military Survey and ourselves,

and it has carried on very much the same traditions.

The most important components of any conference are the members. We seem to get a good mix of the practical, both government and commercial, and the academic; and although the host country is bound to provide most of the membership, the various British organizations try to send a nucleus of people who will stay long enough to provide continuity and not to send too many different representatives each day just because they are the experts on the subject being discussed on that day. The chances are then that these are either too junior or too inexperienced to open their mouths and so become simply part of the audience—and this should not be overdone because all members are invited for what they can contribute and not what they will get out of it; nor should the discussions be too abstruse or specialized. Fortunately the hard core and full-time attenders of the conference are fairly senior members of home and overseas organizations who will give short shrift to the kind of inaudible or incomprehensible talk described so vividly by your correspondent. A few carefully selected members from Europe are also invited, as well, of course, as an American delegation, in order to help us keep up to date with the latest developments.

As far as papers are concerned, a general invitation is issued to Commonwealth survey departments about eighteen months beforehand to suggest papers and authors, and after receiving the replies to this the standing committee lays down the broad lines of the various sessions (fourteen held over a week and a half) and decides what subjects should be discussed and who should be invited to write papers on them. A few uninvited papers are submitted (generally by academics or commercial firms) but they are by no means always accepted; and the total number is limited to about fifty (that is, three or four a session). A few papers are invited from Europe and the USA, usually on advanced techniques.

One of the most essential procedures of the conference is to get almost all the papers in, print them, and circulate them to all conference members before the conference starts. This means not only that the members can be taken to have read them (and are soon silenced in the conference if they obviously have not) but that heads of departments have had time to consult their specialists and get their views on the more abstruse papers. This printing is done quite quickly by the Ordnance Survey by litho directly from the typescripts, and the authors are firmly told to present these and any black-and-white illustrations in the appropriate form. If they have coloured illustrations—and in cartographic papers people of course often do—then they must print enough copies not only for this preliminary circulation of the papers but also for the final proceedings, for which the text of papers and discussion are completely retyped in one text.

Then comes the actual discussion procedure. No author at this conference has ever had the temerity to read his paper in full since the Second World War; he would soon be shouted down by the other members if he did. The authors of the three to five papers being discussed at any one session form a panel with a separate chairman (chosen for his personality and knowledge of the subject much more than for international or political reasons) and in general their introductions (an outline of the chief points or an updating of



the information) are limited to ten minutes each, which takes the session up to a tea or coffee break, giving the whole of the second half of a morning or afternoon session for discussion.

With one exception since 1955 the conference has been held in a big lecture theatre holding about 200 people. The exception—in the Leys School theatre in Cambridge in 1967 with the panel on the stage—was a less successful event than usual. It seems probable that authors of papers easily get too big for their boots and it is much better for them to look up to their audience rather than down. Also, because a crick in the neck is very tiring, the audience are less likely to go to sleep if they are looking down; but few authors of papers go to sleep while actually presenting them. Sitting on the sort of high altar flanked by masses of flowers and national flags favoured by international congresses, together with the discreet attendance of beautiful acolytes, gives speakers and chairmen altogether too great a feeling of their own importance. Moreover the bowl effect of a lecture theatre and the fact that everyone can be heard without using microphones speeds up the procedure (and encourages the occasional wisecrack) because it means that in discussion speakers do not have to come up to the front. The wise conference member who is going to join in the discussion secures a seat at the side of the hall so that at least he has his back to the wall!

The record of the discussions is just as important as the papers themselves (if not more so), and it seems a pity that many excellent conferences should be content to publish proceedings which consist of the papers and a series of pictures of delegates in funny hats but no record of the discussions. In the Commonwealth Survey Conference the discussion is taken down almost verbatim but immediate editing by the speakers concerned is allowed and a final distillation is done by the rapporteurs and editors; this process reduces the discussion by a third or more without eliminating the important points or losing the atmosphere of cut and thrust which is an essential part of a good debate.

Those responsible for organizing the first post-war conference in 1947 still remember with horror the attempt to record the discussion by tape recorder and leave the transcription until after the end of the conference. By the time the transcript had been made all the members had dispersed—all over the world. They were sent the record of what they had said and with one accord so modified this that it made nonsense of comments made by speakers later in the discussion. So the technical secretary was involved in an immense, lengthy and acrimonious correspondence with surveyors all over the Commonwealth before he finally got an agreed version some three years later which could then be printed, only just in time for the next conference.

There can only be two procedures—either tape-record, edit and publish and damn the consequences, or get a draft out within a few hours, pass each speaker his section and insist on getting it back within twenty-four hours. The Commonwealth Survey Conference prefers the last of these procedures and uses a tape recorder and a team of stenographers and rapporteurs (selected conference members) whose job it is to achieve this; it works on the whole. If speaker A makes an error that he would rather forget and speaker B makes capital out of it, they are put together and asked to iron it out so that the record reads consistently. What a man says is often less important than what he meant to say, and certainly less readable than what he would have said with more time to prepare it; so in this way one gets the best of both worlds in an accurate, readable, and reasonably concise record of the interchange of ideas which is after all what a discussion is meant to be.

Your correspondent rightly criticizes the normal kind of conference label, which is usually typed and can therefore only be read by another delegate looking at it so obviously that he might as well ask the name outright and have done

with it. Such labels are quite useless for finding someone whose name one knows but not his or her appearance. Of course typed labels are the easiest but this conference went over to stencilled ones—with letters about 6 mm high—in 1963 and are now using 'Dymotape' (in the larger size). Two benefits were expected, but only one was evident in practice. In cases in which a delegate in the heat of the discussion forgot to give his name (and the chairman forgot to remind him), the rapporteur could read it with the aid of binoculars (your correspondent will be pleased to hear). The other benefit hoped for was that, using a low power microscope, the secretary would be able to read the names on an enlargement of the conference photograph and thus compile a key to it. This did not work because of halation by the white label background into the lettering; the labels could be clearly seen but the lettering had completely disappeared. The secretary had therefore to rely on a combination of personal knowledge, chasing up and asking the names of (or reading the labels of) the few people he did not recognize; once again a simple technique worked best.

One of Hotine's chief contributions (though it had been suggested in the early pre-war days) was to move the conference out of London in 1955 to Cambridge where it has been held ever since. This holds the members together outside the conference hall and allows discussions to continue informally at lunch, or over a pre-lunch or pre-dinner drink, or in one of Cambridge's many pubs or even on a punt on the Cam.

Many symposia have realized the value of using universities or colleges and hold their meetings in similar surroundings, but there are dangers in having the conference hall and the accommodation all under one roof or organizing buses to move delegates between the two. Although in theory this is more efficient and saves the delegates the time and trouble—and exposure to rain and wind—of organizing for themselves four moves a day between the two, in fact it makes things too easy and one gets a conference suffering from sleepiness and—in the later stages—from indigestion and liver because it has not had enough exercise and fresh air and has had too much food and alcohol. Having to organize some things themselves keeps delegates awake and fit.

All conferences nowadays attract a proportion of wives and other dependants who have to be entertained; this is no problem in Cambridge and, in addition to short tours of colleges, one or two all day visits or tours are organized for them together with a popular evening lecture or two on subjects allied to the conference—polar and space travel, building big dams, oil exploration and the like. The RICS and the Government organize receptions before and after the conference, and the instrument makers exhibiting their products give a formal party and several informal (but much more deadly) affairs to selected potential customers. But even with all this it seems unlikely that many of the Commonwealth Survey Conference members would repeat that remark often heard after other conferences: "Of course the sessions were hopeless but one made a lot of useful contacts outside". If a conference cannot do better than that it should not be held.

It would seem that the elements of a successful conference or symposium in order of importance are selection and pre-circulation of papers, one language (if possible), limited numbers (and some selection of members), well planned accommodation outside the capital, sensible recording arrangements, good chairmen and enough, but not too much, opportunity for informal discussion. Given the will and a good lead by its secretary and by a few of the more experienced members, even an international conference could achieve these if it wanted to, and there are signs that this is beginning to happen.

<sup>1</sup> *Sixth United Nations Regional Cartographic Conference for Asia and the Far East, Resolution No. 4, Tehran, 1970 (in the press).*

# On the Possibility of a Human Breast Cancer Virus

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**Electron microscopy of human milk has made possible the classification of three groups of virus-like particles.**

THE virus that causes breast cancer in mice is found in the milk of many inbred strains. If the aetiology of human breast cancer is analogous to that of the mouse, human milk would be a logical source to search for the virus. In thin section electron microscopy of human milk fractions, we<sup>1-3</sup> and others<sup>4</sup> have found particles resembling the mouse mammary tumour virus (B particles) and/or leukaemia virus (C particles). Due to the small number of isolated particles previously observed in thin section, their identification has been questionable, especially because similar structures of cellular origin are frequently observed<sup>5</sup>. We have reported<sup>1,2</sup> the findings of structures identical to B particles in negatively stained preparations of human milk isolates. This initial discovery prompted work in immunology<sup>6</sup> and biochemistry<sup>7,8</sup> that added further evidence for a mouse related mammary tumour virus in human milk.

We report here an electron microscopic examination of an extended series of 381 milk specimens from 263 American women. An improved classification technique and critical analysis has led to grouping of the virus-like particles in human milk into three different types. The significance of these particles in relation to breast cancer in humans is discussed.

The collection of human milk samples was made from two groups of women: ninety were from women with a history of breast cancer in their immediate maternal relatives; 173 with no family history of breast cancer. The method of collection and fractionation of these milks for electron microscopic examination was essentially the same as described before<sup>1</sup>.

## Types of Virus Particles

Figs. 1-5 illustrate the types of particles found in human milk. By thin sectioning, a few particles (Fig. 1) similar in shape and size to those of mouse mammary tumour virus, leukaemia-sarcoma viruses and a virus associated with monkey mammary tumour (M-PMV) were found. The structure

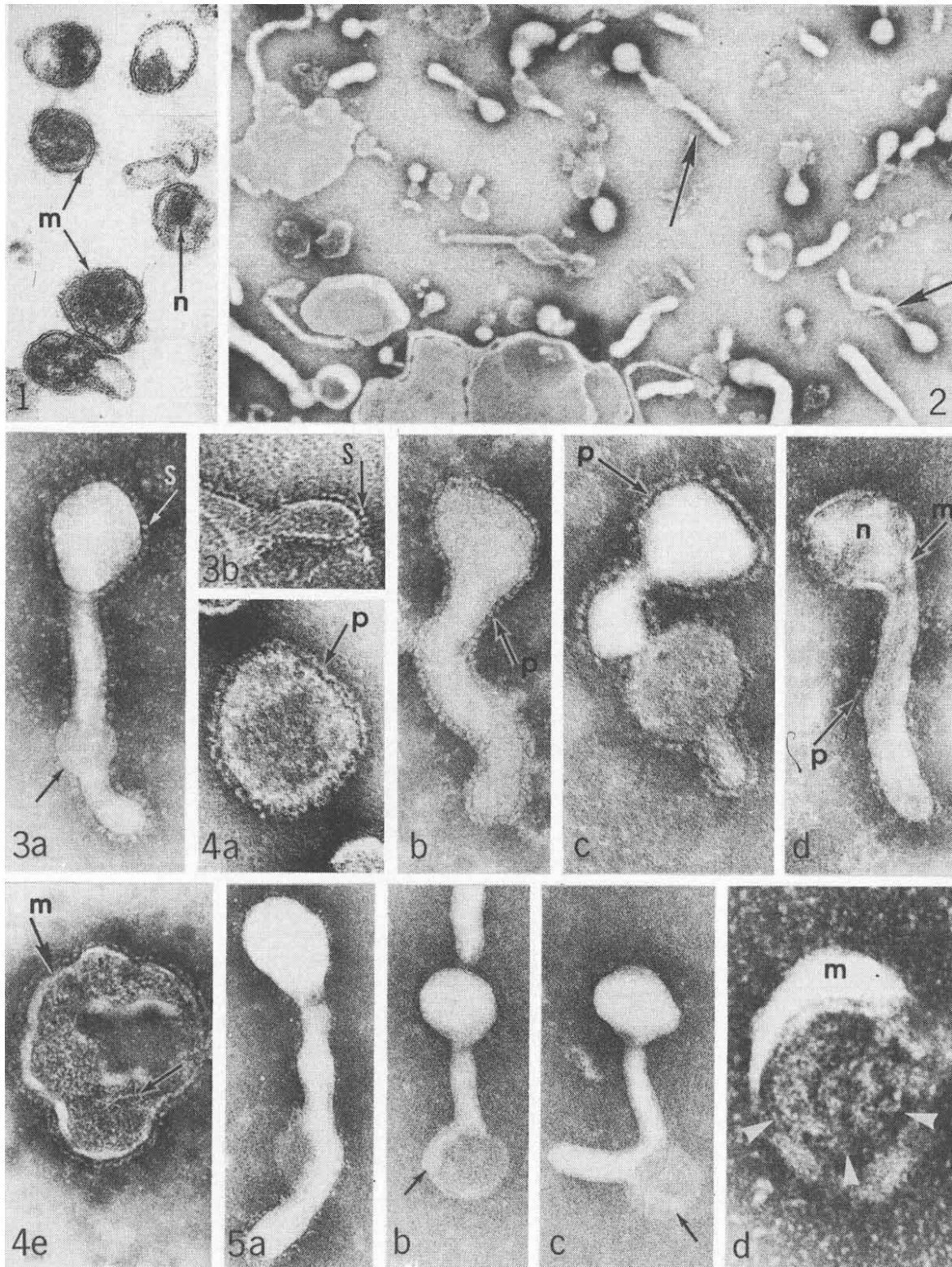
of the nucleoid of these particles, however, resembles more closely those of the M-PMV virus (compare Fig. 1 with Fig. 1c of ref. 9). We cannot positively identify these particles as oncornaviruses but the morphology is consistent with this interpretation.

In negatively stained preparations of B (associated with mouse mammary tumour) or C particles (associated with murine leukaemia), two different forms are generally found: head and tail configurations, and spherical forms; type B has regular spikes on the viral membrane. Positive identification of B particles, even when contaminated with cell fragments, mycoplasma, or other viruses, is not difficult because of their unique surface structure<sup>1,2,10</sup>. For this reason particles similar in morphology to B particles can be readily identified, although infrequently, in human milks (Fig. 3a and b). In negatively stained preparations, C particles cannot definitely be identified unless large numbers of particles of uniform size are seen or particles showing the internal structure<sup>11</sup> characteristic of the oncogenic RNA viruses are observed. Fig. 2 illustrates a typical field where two particles resembling type C and a large amount of cellular debris are seen.

During our 5 yr survey of human milks, irrespective of the milk donor's cancer history, three different types of particles have been found in negatively stained preparations of the milk isolates. Since the abbreviations "B particles" and "C particles" have been used exclusively to represent mouse mammary tumour virus (MuMTV) and leukaemia-sarcoma viruses, we prefer to designate the particles found in human milk differently in order to avoid confusion. Thus (1) "MS-1 particles" (Fig. 3a and b) are defined as those which have surface spikes similar in shape and size and distribution to those of the mouse B particles, and thus are identical in morphology to them. (2) "MS-2 particles" (Fig. 4a-e) are defined as those that have surface projections which are not identical to the surface spikes of B particles. (3) "MS-3 particles" (Fig. 5a-c) are defined as those having heads and tails but no surface projections; thus they resemble negatively stained leukaemia-sarcoma viruses, and the viruses found to be associated with monkey mammary tumour<sup>12,13</sup> and rat mammary tumour<sup>14</sup>. The particles in Figs. 4e and 5d show poorly defined internal structure which resembles to some extent the internal structure of the oncornaviruses.

Type MS-1 particles have been seen in thirteen milk specimens out of 381 and only one or two particles have been found in each case after an extensive search. Thus it has not been





**Fig. 1** A selected field showing virus-like particles from human milk in thin section. The particles were purified by density gradient centrifugation in sucrose, fixed in 1% osmium tetroxide in PBS; sections were stained with uranyl acetate and lead citrate. The membrane (m) of the particles appears smooth; n, the nucleoid ( $\times 81,000$ ).

**Figs. 2-3** Negatively stained (2% sodium phosphotungstate, pH 7.0) particles from human milk. A representative field at low magnification is shown in Fig. 2, where two particles (arrows) resemble type C. Note the large amount of cell debris even though the material was from a band in sucrose gradient in the density region  $1.15-1.18 \text{ g/cm}^3$ . The particle (Type MS-1) in Fig. 3a has head and tail with a series of regular spikes (s) 100 Å long; a bleb at the tail end of the particle is indicated by an arrow. A fraction of a broken particle having similar surface spikes (s) is shown in Fig. 3b. Fig. 2,  $\times 47,700$ ; Fig. 3a,  $\times 157,500$ ; Fig. 3b,  $\times 144,000$ .

**Fig. 4** Examples of type MS-2 particles. The projections (P) of these particles are not uniform. The shape and size of the particles resemble those of the oncornaviruses. The particle in d shows membrane (m), a suggestion of the nucleoid (n) and remnants of broken projections (P). The internal structure of the particle in e has a thread-like appearance (arrow), a characteristic of the nucleoid structure of the oncornaviruses. a,  $\times 162,000$ ; b,  $\times 157,500$ ; c,  $\times 180,000$ ; d and e,  $\times 162,000$ .

**Fig. 5** Examples of type MS-3 particles. The head and tailed particles with smooth membrane often show blebs (arrow). The apparently spherical nucleoid (arrow heads) of the particle in d has the appearance of a ball of yarn. The smooth membrane (m) of the particle and its internal structure resemble very closely those of a type C particle. a,  $\times 117,000$ ; b and c,  $\times 108,000$ ; d,  $\times 216,000$ .



possible to make any quantitative comparison of these particles with respect to MS-2, MS-3 or cellular debris<sup>15</sup>. MS-2 particles have been found in forty-one specimens; the nature of these particles is unknown, but they could possibly represent cellular fragments or type MS-1 in various stages of degradation. It is worth mentioning that there is some similarity between the projections of the avian myeloblastosis virus<sup>16</sup>, a few M-PMV viruses<sup>9,13</sup> and the projections of the MS-2 particles. These MS-2 particles, together with type MS-1, have been designated as type B particles in previous reports from this laboratory<sup>2,3</sup>. It has been found<sup>15</sup> that when mouse milk containing B particles is added to certain human milks, the mouse B particles undergo degenerative changes and may resemble MS-2 particles. Type MS-3 particles have appeared in 134 specimens and in larger quantities. Table 1 shows the occurrence of these particles in human milk from two groups of women. Thus it can be said that particles resembling oncogenic RNA tumour viruses have been found in human milk.

**Table 1** Distribution of the Three Different Types of Virus-like Particles in Human Milk

| Breast cancer history (maternal) | No. of women or specimens | Type of particles |          |          |
|----------------------------------|---------------------------|-------------------|----------|----------|
|                                  |                           | MS-1              | MS-2     | MS-3     |
| History                          | Women 90                  | 4 4.4%            | 15 16.7% | 33 36.7% |
|                                  | Specimens 133             | 4 3.0%            | 20 15.0% | 45 33.8% |
| No history                       | Women 173                 | 9 5.2%            | 20 11.6% | 69 39.9% |
|                                  | Specimens 248             | 9 3.6%            | 21 8.5%  | 89 35.9% |

The morphological similarity of the milk particles to known oncornaviruses<sup>16</sup>, and the consistent finding of RNA-dependent DNA polymerase<sup>7</sup> (see also Table 2) and 70S RNA<sup>8</sup> in some milks containing these types of particles, and their absence from milks without particles, also supports the idea that an oncornavirus is present in some form in certain human milks. If so, which of these three types of particles could be a virus? The enzyme assay and the detection of 70S RNA cannot differentiate between B and C particles; nor can it identify which, if any, of these MS particles is related to breast cancer.

In view of the morphological similarity of MS-1 particles to B particles, the presence of neutralizing antibody in human sera that partially neutralizes the infectivity of mouse mammary tumour virus<sup>6</sup>, the homology of the MuMTV-RNA to the polysomal RNA extracted from human breast tumours<sup>17</sup>, the possibility of the MS-1 particles being a human breast cancer virus is very strong. The possibility remains, however, that the MS-3 particles found most frequently in human milks are the particles in some way related to human breast cancer, and MS-1 particles are merely "passenger" agents which have no active role in the establishment of the neoplasm. This situation would be analogous but opposite to mammary cancer in mice, where B particles are abundant and a very few C particles can be found (N. H. S. and D. H. M., unpublished results). Mice having both B and C particles always develop mammary cancer but only occasionally leukaemia. The finding of C type particles associated with monkey mammary tumour and rat tumour supports such a hypothesis. This raises another question, however, as to why, by thin section electron microscopy of the pellets obtained from human milk, one does not find a comparable number of particles resembling either B or C. It is possible that only a small number of MS-3 particles are really virus and the rest of them are cell fragments which, in negative staining, may look like virus.

Table 1 shows that the incidence of occurrence of these three types of particles in milks from women with or without a familial history of breast cancer is similar. This suggests that if any one of these classes of particles represents an oncogenic virus related to human breast cancer, then the virus may be widely distributed. Such a hypothesis has been advocated in the case of breast cancer in mice<sup>18</sup>. Before a definite conclusion can be reached on the distribution of the various types of particles in milks from various sources, much more carefully standardized work must be done.

**Table 2** Occurrence of the Three Different Types of Virus-like Particles in Human Milk and the RNA-dependent DNA Polymerase Activity

| Milk samples from women with a family history of breast cancer | Type of particles |      |      | RNA-dependent DNA polymerase * |
|----------------------------------------------------------------|-------------------|------|------|--------------------------------|
|                                                                | MS-1              | MS-2 | MS-3 |                                |
| 1                                                              | —                 | +    | +    | +                              |
| 2                                                              | —                 | —    | +    | +                              |
| 3                                                              | —                 | —    | +    | +                              |
| 4                                                              | —                 | —    | —    | —                              |
| 5                                                              | —                 | +    | +    | —                              |
| 6                                                              | —                 | —    | +    | —                              |
| 7                                                              | —                 | —    | —    | —                              |
| 8                                                              | —                 | —    | —    | —                              |
| 9                                                              | +                 | +    | +    | +                              |
| 10                                                             | —                 | —    | —    | —                              |

| Milk samples from women with no cancer history | Type of particles |      |      | RNA-dependent DNA polymerase * |
|------------------------------------------------|-------------------|------|------|--------------------------------|
|                                                | MS-1              | MS-2 | MS-3 |                                |
| 1                                              | —                 | +    | +    | +                              |
| 2                                              | —                 | —    | +    | —                              |
| 3                                              | —                 | —    | +    | +                              |
| 4                                              | —                 | —    | +    | +                              |
| 5                                              | —                 | —    | —    | —                              |
| 6                                              | —                 | —    | —    | —                              |
| 7                                              | +                 | +    | +    | +                              |
| 8                                              | —                 | —    | —    | —                              |
| 9                                              | —                 | —    | +    | —                              |
| 10                                             | —                 | —    | +    | —                              |

\* Provided by Dr J. Schlom of the College of Physicians and Surgeons; the results have been reported elsewhere<sup>8</sup>. Milks were processed and purified bands from the 1.17 to 1.19 region in sucrose equilibrium density gradients were assayed for the RNA-dependent DNA polymerase as previously described<sup>7</sup>.

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# Evidence for a Chemical Discontinuity near the Basalt–Andesite Transition in Many Anorogenic Volcanic Suites

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**There is a chemical gap between basalts and intermediate lavas in numerous anorogenic volcanic suites. The principal cause is thought to be *in situ* closed-system high-pressure partial crystallization of bodies of basaltic magma, with release of evolved liquid residua only when the masses are approximately half solid. This implies that most tholeiitic andesites, hawaiites and trachyandesites are generated at upper mantle depths, not in high-level subvolcanic magma reservoirs.**

place in a subvolcanic magma chamber which is essentially analogous to those postulated to form layered intrusions. The crystal sedimentation that is supposed to take place during the consolidation of layered intrusions seems to be a very efficient method of separating precipitated crystals from residual liquids. Thus, if the latter were sampled at frequent intervals by volcanic eruptions, the resulting lavas should show continuous chemical variation. Daly's<sup>1</sup> conclusion that on oceanic islands the basalts and trachytes greatly outnumber the intermediate lavas and Chayes's<sup>2</sup> re-emphasis of this point are not the subject of this article, which is concerned with lavas all on the mafic side of the "Daly Gap".

During a chemical study of the Lower Eocene lavas of North Skye, Scotland, sixty major element chemical analyses of basalts and hawaiites were made<sup>3</sup>. Inspection of the analyses and their plotted positions on various diagrams revealed that, although both basalts and hawaiites were abundantly represented, there was a small but distinct chemical discontinuity between these two groups. At first it was assumed that the gap resulted from insufficient field sampling. But this hypothesis failed to explain why intermediates between basalt and hawaiite were not at least as common as the latter, if a high-level "layered intrusion type" fractional crystallization model was valid for these lavas. In search of alternative explanations, I have made a survey of published accounts of chemical variation in anorogenic volcanic suites. This survey

Fractional crystallization is generally favoured as the mechanism whereby suites of anorogenic lavas come to vary chemically from basalt and basanite, through intermediate lavas, to rhyolite, trachyte and phonolite. Many authors, especially those who find layered gabbroic xenoliths in the lavas they describe, appear to envisage this process as taking

has produced the totally unexpected conclusion that a chemical gap near the basalt-intermediate lava transition seems to be a common worldwide phenomenon.

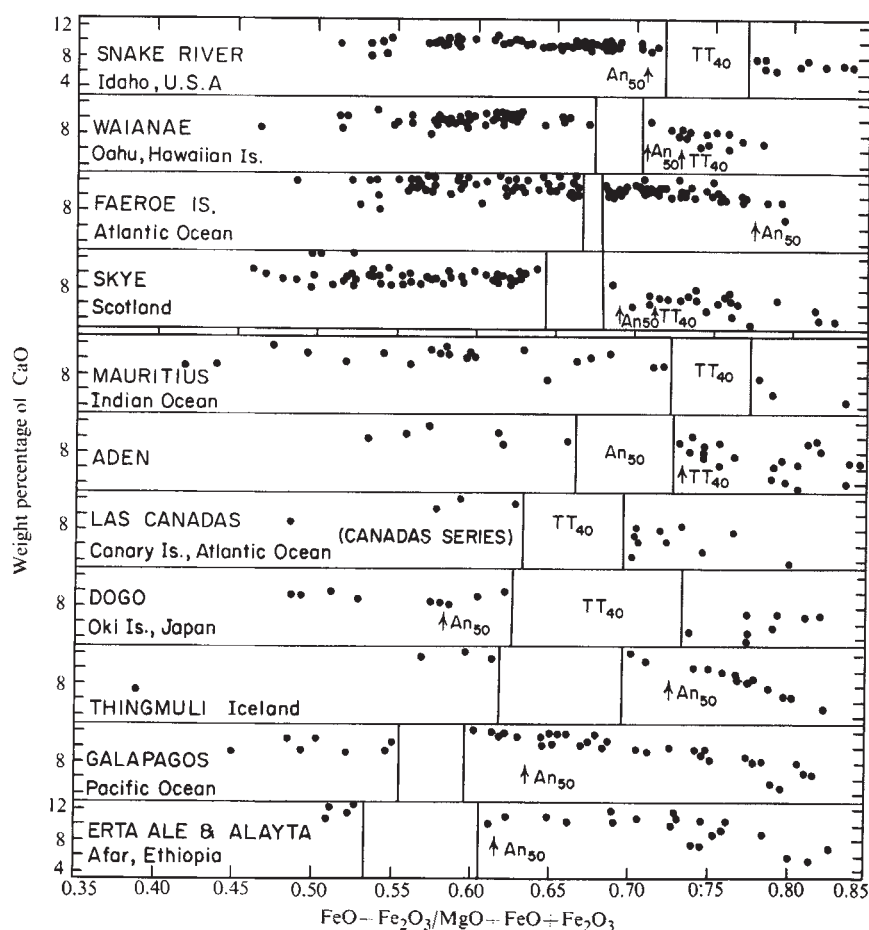
## Definition of Basalt and Andesite

The wealth of subdivisions of volcanic rocks used in their classification is an embarrassment when attempting to define a widespread feature. The following simplifications are used in this study. The term "basalt" incorporates tholeiitic, transitional and alkaline basalts, trachybasalts and basanites. The term "andesite" includes tholeiitic andesites (icelandites),

melting studies<sup>6</sup> at 1 atmosphere that basalts and andesites plotted against this simple index are effectively aligned in order of decreasing liquidus temperatures as iron enrichment increases. The reason for choosing CaO as ordinate will become apparent below. Note that the volcanic suites depicted on Fig. 1 come from localities scattered over the major continents and ocean basins of the world. They include examples of both central volcanoes and fissure-erupted lava floods, the former situated on both continental and oceanic crust<sup>3,7-18</sup>.

The basalt-andesite transition, in terms of normative plagioclase, Thornton-Tuttle index, or both, is indicated for each suite. It then becomes apparent that in each of these varied volcanic suites there is a distinct chemical discontinuity at or near the basalt-andesite transition. The only major

**Fig. 1** CaO plotted against  $(\text{FeO} + \text{Fe}_2\text{O}_3)/(\text{MgO} + \text{FeO} + \text{Fe}_2\text{O}_3)$  for lavas from eleven anorogenic volcanic suites. The points at which each suite passes a normative plagioclase value of  $\text{An}_{50}$  and/or Thornton-Tuttle index of 40 are marked on the diagram. In some suites, where one or other of these points is not clear-cut, the best-fitting point has been marked.



hawaiites, nepheline-hawaiites, trachyandesites and feldspathoidal-trachyandesites<sup>4</sup>; calc-alkaline andesites are excluded because of their orogenic environments. Later this point is discussed further. The boundary between basalt and "andesite" is placed at the CIPW normative plagioclase value of  $\text{An}_{50}$ . This single discriminator is not very successful with strongly alkalic lavas. For these a Thornton-Tuttle index<sup>5</sup> value of 40 gives satisfactory results<sup>4</sup>.

## Examples of the "Basaltic Andesite" Gap

Chemical data from eleven well known anorogenic volcanic provinces are shown in Fig. 1. The iron-magnesium ratio is used as abscissa, because it is known from experimental

exception to this is the Faeroe Islands where a gap appears well within the basalt range on Fig. 1. These lavas are iron-rich, alkali-poor tholeiites<sup>16</sup>. If the basalt-andesite discriminator chosen had been one incorporating Fe/Mg variation, rather than normative plagioclase alone, the gap in the Faeroe lavas would have fallen much closer to the basalt-andesite transition.

The data of Fig. 1 have been split into two groups. At the top, the suites of Snake River, Waianae, Faeroe Islands and Skye are examples of lava piles from which abundant specimens have been analysed. Areas represented by somewhat fewer analyses are plotted in the bottom part of the diagram.

The discontinuities marked on Fig. 1 are clearly but one of several gaps in those suites where data are relatively sparse. As such they have hitherto passed unnoticed in the literature.

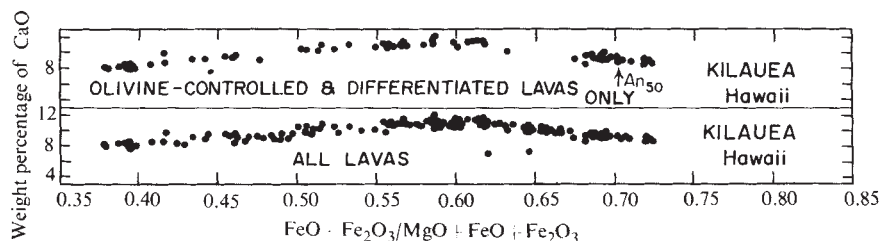


Indeed, the authors of several of the papers used as sources for Fig. 1 stressed the chemical continuity of the suites they were describing. The existence of a "basaltic andesite" gap becomes apparent only when data from a number of suites are plotted together, as here. Three other points add vital reinforcement to the proposal that this discontinuity is real and not a product of inadequate sampling or errors in the chemical analyses. First, Fig. 1 shows that it is in the suites with the most abundant data (Snake River, Waianae, Faeroes, Skye and Galapagos) that the gap is most clearly defined. Second, CaO does not always vary continuously across the gap but instead tends to show a discontinuity (see especially Skye; but also, less clearly, Snake River, Faeroes, Waianae, Las Canadas and Galapagos). Third, analytical errors can be discounted as the cause of the gaps, because these errors would generate a scattering of the points that would fill the empty zones marked on Fig. 1. Other clear demonstrations of the gap may be had by studying the multitudinous variation diagrams plotted for each suite in Fig. 1 in the papers from which these data have been abstracted<sup>7-18</sup>.

The literature survey has yielded only four examples of well sampled lava suites that vary through basalt and andesite without any clear discontinuity. These are Gough Island<sup>19</sup> and St Helena<sup>20</sup>, Atlantic Ocean; Tahiti<sup>21</sup>, Pacific Ocean; and Otago<sup>4</sup>, New Zealand. Of these, Baker<sup>22</sup> has produced evidence other than the chemical variation of the lavas for the

or depths of their operation. For simplicity, such ill defined processes as gas transfer will be ignored and it will be assumed that the lavas were generated by mechanisms essentially involving crystals and liquid alone. It then appears that a key to how the gap might be formed is provided by recent work on the lavas of Kilauea volcano, Hawaii. It has long been supposed that the Kilauea lavas are an excellent example of continuous variation in a volcanic suite, brought about solely by the fractional crystallization of basalt with efficient crystal-liquid separation<sup>34</sup>. Recent work by Wright and Fiske<sup>35</sup> has generated a much more complex model, in which (1) summit-erupted lavas are fed from successive, unique, mantle-derived magma batches, modified only by olivine addition or subtraction; (2) parts of summit magma batches are emplaced in rift zones on the flanks of the volcano where they crystallize undisturbed until about 50–60% solid; (3) at this point the solid crystal meshwork of these magma pockets is sufficiently rigid to pull apart without disaggregation when stressed, allowing the interstitial liquid to migrate into low-stress zones as pools of strongly differentiated magma (the operation of this process on a small scale in the crusts of crystallizing lava lakes allows highly evolved oozes to filter from the semisolid crust into drill holes<sup>35,36</sup>); (4) later mantle-derived batches of relatively unevolved magma migrate into the flanks from beneath the summit area and mix with pockets of the evolved magma, to produce a range of hybrids.

Fig. 2 CaO against  $(\text{FeO} + \text{Fe}_2\text{O}_3)/(\text{MgO} + \text{FeO} + \text{Fe}_2\text{O}_3)$  for lavas from Kilauea volcano, Hawaii. Both scales are the same as in Fig. 1.



existence of a large, single, high-level magma chamber beneath St Helena. McBirney and Aoki<sup>21</sup> have proposed that the chemistry of the Tahiti lavas fits better to a model involving separation of high-pressure phases at upper-mantle depths than to one invoking "layered intrusion type" fractionation in a high-level magma chamber. Coombs and Wilkinson<sup>4</sup> have demonstrated that the Otago lavas comprise a number of separate lineages which probably should not be treated together as a single series.

Several other lava suites show a prominent discontinuity at or near the basalt-andesite transition but were not plotted on Fig. 1 because only very few analyses define one or other side of the gap. Examples are Tristan da Cunha<sup>23</sup>, Atlantic Ocean; East Island<sup>24</sup>, Indian Ocean; Iki Islands<sup>25</sup> and Takasayama<sup>26</sup>, Japan; Mt Etna<sup>27</sup>, Sicily; and Taos<sup>28</sup>, New Mexico. Other lava suites provide negative evidence for the gap by failing to cross it. Examples of these where all the lavas are basalts (using the criteria defined above) are Baffin Bay<sup>29</sup> and the Karroo lavas of Basutoland<sup>30</sup>. Examples where they are all andesites are Ascension Island<sup>1</sup>, Atlantic Ocean; Moroto<sup>31</sup>, Uganda; Steens Mt<sup>32</sup>, Oregon; and Nandewar<sup>33</sup>, New South Wales, Australia.

## Causative Mechanism of the Discontinuity

If the existence of this "basaltic andesite" gap is accepted, an attempt must next be made to determine the process or processes that may have generated it and the probable depth

Fig. 2 shows Kilauea lava analyses plotted in the same manner as those on Fig. 1, but in two versions. The bottom section includes all known analyses. The top one excludes the compositions identified by Wright and Fiske<sup>35</sup> as formed by mixing of summit and flank magmas. A close similarity between version two and the lava suites showing a "basaltic andesite" gap on Fig. 1 is immediately apparent.

Wright and Fiske's model of pockets of magma crystallizing quietly in the flanks of Kilauea is one that makes them essentially analogous to sills. With this point in mind the literature on sills was searched. Fig. 3 shows data for two sills for which an exceptional number of analyses is available<sup>37-39</sup>. The Palisades Sill shows a chemical discontinuity near the basalt-andesite transition that is as marked as any in Fig. 1. The Prospect Sill analyses show a gap less clearly but are nevertheless far from the continuous series that "layered intrusion type" crystal-liquid separation should produce.

Authors of modern studies of sills<sup>38,39</sup> mostly minimize the importance of crystal sedimentation in the magma body as an agent for producing the chemical variation observed in the rocks and instead tend to stress the quiet inward migration of residual liquids from the growing crystal meshwork around the walls of the pluton. Shaw<sup>40</sup> has noted that, when Kilauea basalt is about half crystalline, it is able to withstand shear stresses of ~100 bar for periods of at least several minutes without apparent failure. If the same is true for a semisolid basaltic sill, it may be that the application of a specific substantial stress, such as an earthquake shock, is required to force

the residual liquid out of the crystalline meshwork. When such a process occurred, a relatively clean separation of crystals and liquid might result, giving a discontinuity on a diagram such as Fig. 3.

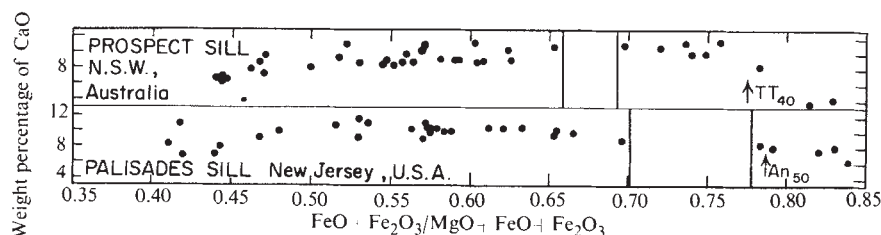
Returning now to lavas, I propose that the "basaltic andesite" gap revealed on Fig. 1 is generated thus: basalt magma is formed by partial fusion of the upper mantle and some of it immediately penetrates to the surface, to be erupted from volcanoes or fissure fields in a relatively unmodified condition, except for chemical variation resulting from high-pressure partial fusion trends and low-pressure olivine control. Some magma batches stagnate on the way to the surface and form sill-like or irregular bodies that generally do not establish convective circulations, but instead crystallize tranquilly until about half solid. At this critical point (which is documented at low pressure by studies of the Kilauean lava lakes<sup>35,36</sup>) the crystalline meshwork fractures rather than flows in response to stress, enabling the evolved interstitial liquid on the andesitic, low-temperature side of the gap to escape and to be erupted with little further modification. The vital point of this model is that it explains the "basaltic andesite" gap as the product of a repetitive, reproducible discontinuous sampling mechanism superimposed on the crystal-liquid processes causing the gradational chemical variation of the magma at depth. This mechanism allows only two types of liquids from the differentiation process to reach the surface—samples of relatively

## Depth of Generation of the Intermediate Lavas

In the sills (Fig. 3) the "basaltic andesite" gap was presumably formed under near-surface conditions. Wright and Fiske<sup>35</sup> have given detailed evidence that this is also so in Kilauea (Fig. 2). It seems most improbable to suppose that Kilauea is a unique volcano. Therefore, it might be expected that in other instances the gap can form in near-surface conditions within volcanic edifices. But none of the cited examples seems to be explicable exclusively in terms of low-pressure processes. On the other hand, several of them show evidence that the link between their basalts and andesites should be sought at high pressures. Thus, in the Waianae<sup>14</sup> and Etna<sup>27</sup> suites the basalts are tholeiites and the andesites are alkalic (hawaiites), lying on opposite sides of the low-pressure thermal divide near the critical plane of silica under saturation<sup>42</sup>. In these cases a parent-daughter relationship across the "basaltic andesite" gap would be confined to pressures above about 7 kbar (ref. 43). The same reasoning applies to Galapagos<sup>8</sup>, where the basalts are alkalic and the andesites tholeiitic.

In the Faeroe<sup>16</sup> and Thingmuli<sup>9</sup> suites it appears probable that the lavas on opposite sides of the discontinuity are also separated by the hypersthene-gabbro thermal divide<sup>43</sup>, which is stable at pressures between about 5 and 15 kbar. Finally

Fig. 3 CaO against  $(\text{FeO} + \text{Fe}_2\text{O}_3)/(\text{MgO} + \text{FeO} + \text{Fe}_2\text{O}_3)$  for dolerites from two sills. Both scales are the same as in Fig. 1.



unevolved basalt and the highly evolved interstitial liquids of the *in situ* crystallization stage.

An implication of the hypothesis presented here is, of course, that the more evolved iron-enriched andesitic lavas must have been erupted after any basalts which sampled the parent magma that had partially crystallized to form the derivatives. This restriction would not have to apply to all the lavas in a given suite, only to subgroups that were closely related in space and time. Many of the source references<sup>7-28</sup> used in this study give insufficient details on the time relations of their analysed samples for any firm conclusions to be drawn on this point. In the following cases, however, the andesites were definitely erupted after the basalts: Waianae<sup>14</sup>, Snake River<sup>17</sup>, Skye<sup>3</sup>, Las Canadas<sup>11</sup>, Taos<sup>28</sup> and Etna<sup>27</sup>. Skye is a complex case where the sequence is repeated. Thus, in eastern North Skye the upward lava succession is basalt, hawaiite (time gap), basalt, hawaiite. Only in the Faeroe Islands<sup>16</sup> do the more evolved lavas clearly underlie the less evolved ones, but in this instance the bottom of the lava pile is not exposed. It may be that further unevolved basalts occur at depth, thus duplicating the situation found in Skye, a part of the same volcanic province.

At this point it will be clear why a stepwise fractional partial melting scheme, such as has been discussed by Press<sup>41</sup>, was not considered as a possible causative mechanism for the "basaltic andesite" gap. Andesites from a partial melting process would be erupted before their associated basalts.

A different line of evidence, suggesting generation of the gap at high pressures, is found in the Skye and Snake River lava suites. Melting experiments<sup>3,17</sup> show that in each case the 1-atmosphere liquidus phases in lavas bordering both sides of the discontinuity are olivine and plagioclase, with clinopyroxene melting at considerably lower temperatures. In contrast, the chemistry of both suites shows that pyroxene (probably calcium-rich clinopyroxene) would have to be removed in substantial amounts from the basalts to derive the andesites across the gap. Pyroxenes would be likely anhydrous liquidus or near-liquidus phases in these basalts at pressures above about 8 kbar (ref. 42). Experimental study of a Snake River basalt confirms this point. It will now be clear why CaO was chosen as the ordinate for Fig. 1. The behaviour of this oxide across each "basaltic andesite" gap gives a good clue as to whether precipitation of Ca-rich clinopyroxene or Ca-poor orthopyroxene may have played a predominant role in any specific example.

## Intermediate Lavas from the Upper Mantle

I propose two specific variants of the high-pressure process. The first is exemplified by Waianae, where the basalt-andesite transition occurs late in the history of the volcano, after most of the lavas have been erupted. It is suggested that, as the upper mantle disturbance that gave rise to the volcano waned, pools of basaltic magma became stagnant in the zone where



they were collecting and began to crystallize. When they were about half solid, they ruptured and the andesitic interstitial liquid was forced upwards and extruded. Of course, the transition from tholeiitic shield-building basalts to late-stage alkalic cappings in several Hawaiian volcanoes has already been ascribed to high-pressure crystallization of the tholeiite<sup>44</sup>. The main departures in this study are to focus attention on a single exceptionally well studied volcano, Waianae, rather than incorporating data from the whole group, and hence to show that in this specific instance the transition is from tholeiitic basalt, not to alkali basalt, but to hawaiite (andesite).

The second proposed model concerns suites such as Skye, Las Canadas and Etna, where the basalt-andesite transition comes early in the lava sequence. I suggest that this results from relatively minor upper mantle disturbances, in which the temperature rises slowly. Thus, when mantle fusion begins, the latent heat of melting of the magma acts as a heat sink, preventing any further temperature rise in that portion of mantle for a while. The delay in turn decreases the thermally induced stresses that speed the magmas to the surface and, as the mantle is heating only slowly, relatively few flows of early basalt are erupted at the surface. Later batches of magma come to stagnate and partially crystallize in cooler mantle above their generation zone. In both models the most plausible agents for disrupting the half-crystalline magma bodies are the stresses associated with heating, cooling and magma production in the mantle beneath a volcanic area.

There is already a substantial body of opinion favouring the hypothesis that calc-alkaline andesites originate in the upper mantle<sup>45-47</sup>. Wilkinson and Binns<sup>48</sup> have described a hawaiite from New South Wales, Australia, containing xenocrysts and xenoliths of apparently high-pressure origin. They suggested that this lava had originated in the upper mantle. Here it is tentatively proposed that most tholeiitic andesites, hawaiites, nepheline-hawaiites and trachyandesites are generated by the partial *in situ* crystallization of basic magmas at upper mantle depths.

## Lithospheric Plates

The word "anorogenic" has been used throughout this account in its classical petrology sense. It is not a term that fits easily into the conceptual framework of plate tectonics, whose *afficionados* tend to explain all volcanic phenomena as manifestations of the pulling apart and marginal jostling of lithospheric plates and the passage of their inner parts over rising plumes of convecting asthenosphere<sup>49</sup>. The only major constraint that the lithospheric plate concept would seem to place on the magma-genesis models proposed here is that they must occur within the plates, to prevent decoupling between the volcanoes and their lava sources. If this is so, it is difficult to see how the calc-alkaline lava suites of orogenic belts may legitimately be ignored in this discussion, because, whatever unique tectonic circumstances may or may not have controlled the origin of these magmas, there seems to be no good *a priori* reason to suppose that the magmatic plumbing of a calc-alkaline volcano at moderate depths, within a rigid lithospheric plate, might be significantly different from other types of volcano elsewhere.

Other specific reasons have forced the exclusion of calc-alkaline lavas from the survey outlined above. First, calc-alkaline volcanic suites that include both basalts and andesites are notoriously rare; second, calc-alkaline lavas are characterized by very limited variation in Fe : Mg ratio, relative to SiO<sub>2</sub> and alkalis, and cannot be aligned in any reasonable parent-daughter sequence on a diagram using an Fe : Mg ratio abscissa; third, many andesites contain abundant phenocrysts of very calcic plagioclase, which cause interpretive problems<sup>47</sup> when normative plagioclase is used as a basalt-andesite discriminator.

In spite of these difficulties, two calc-alkaline sequences have

have been located which show a clear basalt-andesite (normative An<sub>50</sub>) gap that could be demonstrated well by plotting CaO against (Na<sub>2</sub>O + K<sub>2</sub>O)/(MgO + FeO + Fe<sub>2</sub>O<sub>3</sub>). The suites are Talasea<sup>50</sup>, New Britain, and Semisopochnoi<sup>51</sup>, Aleutian Islands. Clearly there is need for a careful comprehensive resurvey of the chemistry of calc-alkaline volcanic suites before they are excluded from the hypotheses outlined in this paper.

The persistent questioning of P. J. Modreski and M. J. O'Hara about the basalt-hawaiite gap in Skye triggered the line of thought reported here. I thank R. A. Bailey, F. Chayes, I. Kushiro, R. W. LeMaitre, H. R. Shaw, T. L. Wright and H. S. Yoder, jun., for their reviews of the manuscript.

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# LETTERS TO NATURE

## PHYSICAL SCIENCES

### Variations in Australian Upper Mantle Structure, from Observations of the Cannikin Explosion

It has been established that P and S times to stations in shield areas are significantly earlier than times to other continental regions at similar epicentral distances<sup>1-4</sup>. The observed differences, called "station anomalies", are largely produced by variations in the properties of the low velocity layer within the upper mantle<sup>5,6</sup>, thus providing one of the few means available for estimating such variations. Except for some studies based on the dense seismic network in California<sup>7-9</sup>, however, detailed investigations of upper mantle structure by this method have been precluded by the relatively sparse distribution of permanent stations.

Muirhead and Simpson<sup>10</sup> have recently developed portable seismic stations which can operate unattended for long periods. These instruments have enabled us to study the variation of P anomalies across the eastern boundary of the Australian shield, using the nuclear explosion Cannikin on Amchitka Island as a signal source. The position of the shield boundary is not accurately known, but a previous study of P times to Australian stations from Longshot<sup>11</sup> had indicated a significant change in the pattern of anomalies between stations of the Australian National University network in New South Wales and those of the Adelaide University network in South Australia. Although the firing date for Cannikin (2200 h on November 6, 1971) was not announced until a few days beforehand, the capability of the instruments for long term unattended operation allowed us to deploy them across a 1,700 km traverse about 3 weeks before the explosion took place. In conjunction with some of the permanent network

stations, ten portable stations were set up at intervals of 100 to 150 km between Canberra and Maralinga, along a line approximately at right angles to the shot direction (Fig. 1a). The positions of some stations were chosen to coincide with those of a recent magnetic variometer experiment by Gough, McElhinny and Lilley (personal communication). The P signal from the explosion was well recorded at all stations: an example is shown in Fig. 2. Onset times were measurable to an accuracy of about 0.05 s at the portable stations, and to better than 0.2 s at the permanent stations. Epicentral distances and travel times were calculated from the official shot data, and details are given in Table 1, including station anomalies calculated both from the 1968 tables<sup>12</sup> and the P table of Hales *et al.*<sup>13</sup>. The two sets of anomalies, reduced to a common baseline, are plotted in Fig. 1b. Thus reduced, the difference between the two sets is 0.07 s at Maralinga and less than 0.05 s at all other stations. Errors introduced by the application of distance corrections appear therefore to be insignificant.

It is unlikely that the data have been contaminated to any significant extent by effects in the vicinity of the source. The azimuthal variation found for the Longshot source anomaly<sup>14,15</sup> corresponds to a change of less than 0.05 s between Canberra and Maralinga. There is no measurable difference between the relative times to Canberra and Adelaide from Longshot and Cannikin although the two explosion sites are 6 km apart, and we infer that any effect of structural irregularities near the source has been negligible. We believe also that the results have not been appreciably affected by variations in crustal structure beneath the stations. Estimates of crustal thickness beneath south-east Australia and the Maralinga-Adelaide region do not differ by more than a few km<sup>16-18</sup> and the observed differences in crustal velocity between the two regions are insufficient to account for a difference of more than about 0.2 s in the anomalies. As Hales *et al.*<sup>6</sup> pointed out, moreover, differences in crustal structure will not produce anomalies of

Fig. 1 *a*, The Cannikin traverse. Shaded and unshaded circles represent permanent and portable stations. The dashed line indicates the eastern boundary of Precambrian rocks<sup>19</sup>. *b*, Differences between P station anomalies along the traverse, calculated from the 1968 P tables<sup>12</sup> (○) and the P table of Hales *et al.*<sup>13</sup> (×). The smoothed curve has been derived by taking means of adjacent points.

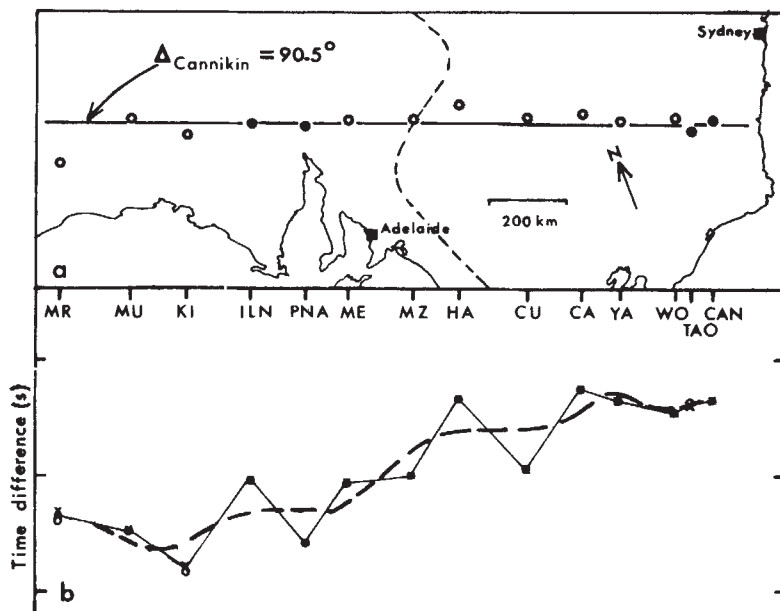






Fig. 2 The P onset from Cannikin and radio time signals recorded on magnetic tape by the portable station at Curragh.

this order without the accompaniment of disturbances in the gravity field much greater than those observed here.

Nevertheless, the scatter between anomalies at adjacent stations is quite large, with an average value of 0.4 s. It must be concluded that this is a consequence either of local inhomogeneities in the upper mantle beneath the stations or of minor irregularities in the velocity structure along the entire path (or a combination of the two). It should be possible to resolve this question by studying arrivals from a number of events at some of the stations. An interesting feature of the scatter is that it is least marked near the eastern end of the traverse, in a region that has been subjected to recent uplift.

In spite of the scatter, Fig. 1b clearly indicates a trend towards more negative anomalies (corresponding to earlier arrivals) in a westerly direction. The trend appears to be most pronounced near the centre of the traverse. This feature is emphasized when first-order smoothing is applied by taking means of adjacent points, as shown in the figure. Following Hales *et al.*<sup>6</sup>, the change in gradient may be interpreted as a wedging out of the lower velocity layer in the upper mantle as the shield boundary is approached. Fig. 1a shows that this occurs close to the line indicating the eastern limit of exposed Precambrian rocks<sup>19</sup>.

The western end of the presumed "wedge" happens to coincide with a NW-SE trending anomaly in the vertical component of the magnetic variation field, found by Gough, McElhinny and Lilley (personal communication). Although the magnetic variation data have not yet been fully interpreted, it may be noted that Hales *et al.*<sup>6</sup> found a similar coincidence between one end of their postulated "wedge" and a magnetic variation anomaly found by Schmucker<sup>20</sup> and subsequently mapped by Reitzel *et al.*<sup>21</sup> and Porath *et al.*<sup>22</sup>.

Apart from this central feature of the anomaly pattern, the data of Fig. 1b also suggest a gradual decrease in the anomalies from east to west. The Longshot data suggest that an additional decrease of about 1 s takes place between Maralinga and Kalgoorlie, situated some 950 km further west in the Archaean portion of the shield. The possibility arises that the decrease is associated with an ageing process within the upper mantle,

corresponding to a gradual disappearance with time of the low velocity layer. Other geophysical observations in Australia seem to have an association with the ages of basement rocks, notably the occurrence of very low heat-flow values in the Archaean shield, and very high values in south-east Australia<sup>19</sup>. Cleary<sup>18</sup> has also drawn attention to an east-west increase in  $P_n$  velocities across the continent (not in itself sufficient to account for the variation in P anomalies).

The apparent relationship between changes in the velocity structure of the upper mantle and the ages of crustal rocks has an important bearing on questions of continental growth. In particular, the implied change in the character of the low velocity layer across the shield boundary appears to be compatible with models of accretion such as that proposed recently for south-east Australia by Oversby<sup>23</sup>.

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Table 1 Station Data

| Station             | Distance*<br>(deg) | Travel<br>time* (s) | Station anomaly† (s) |       |
|---------------------|--------------------|---------------------|----------------------|-------|
|                     |                    |                     | (1)                  | (2)   |
| Maralinga (MR)      | 91.34              | 786.13              | -0.58                | -0.92 |
| Mulgathing (MU)     | 90.47              | 781.99              | -0.70                | -1.10 |
| Kingonya (KI)       | 90.77              | 782.97              | -1.02                | -1.40 |
| Island Lagoon (ILN) | 90.45              | 782.30              | -0.23                | -0.63 |
| Partacoona (PNA)    | 90.55              | 782.20              | -0.78                | -1.18 |
| Melton (ME)         | 90.44              | 782.17              | -0.27                | -0.67 |
| Mazar (MZ)          | 90.30              | 781.62              | -0.20                | -0.61 |
| Harcourt (HA)       | 90.01              | 780.90              | +0.47                | +0.05 |
| Curragh (CU)        | 90.36              | 781.90              | -0.12                | -0.53 |
| Carrathool (CA)     | 90.21              | 781.86              | +0.54                | +0.13 |
| Yanco (YA)          | 90.38              | 782.59              | +0.45                | +0.05 |
| Woodstock (WO)      | 90.36              | 782.39              | +0.37                | -0.04 |
| Talbingo (TAO)      | 90.79              | 784.40              | +0.37                | -0.01 |
| Canberra (CAN)      | 90.34              | 782.40              | +0.45                | +0.04 |

\* Calculated from shot coordinates and origin time given for Cannikin by NOAA (PDE card 82-71).

† Difference between observed time and time calculated from (1) 1968 P tables<sup>12</sup>, (2) P table of Hales *et al.*<sup>13</sup>, with ellipticity correction applied.

## Are Quasars Local or Cosmological?

THE debate about the distances of the quasars resembles in many ways the nineteenth century debate on the nebulae. One of the chief defining characteristics used, that of being stellar in appearance, is apparent rather than intrinsic. And rather compelling arguments have been advanced to show both that quasars are at the large distances implied by a cosmological interpretation of their redshifts (continuity with radio-galaxies<sup>1,2</sup>, association with cluster of same redshift<sup>3</sup>) and that they are comparatively weak, nearby objects (continuity with Seyfert nuclei<sup>4,5</sup>, associations with foreground galaxies<sup>6</sup>, the anomalous 1.95 redshift<sup>7</sup>, rapid lateral motions<sup>8</sup> in 3C 273 and 279, anisotropic distribution on the sky<sup>9</sup>).

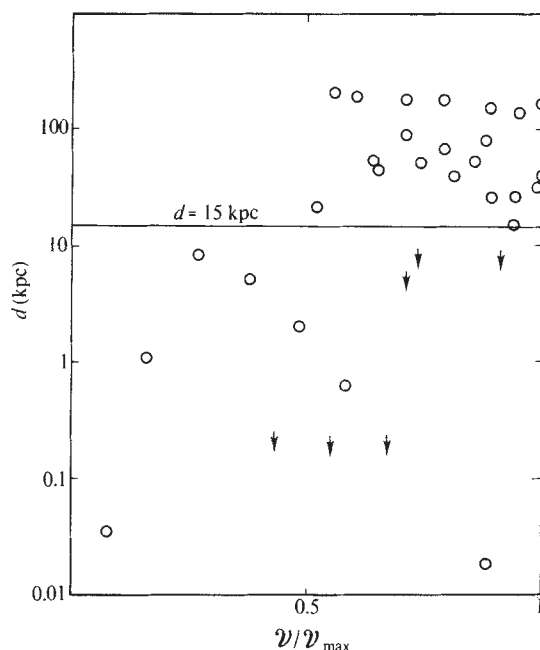


Fig. 1 Largest radio dimension,  $d$ (kpc), against  $V/V_{\max}$ , for 35 3CR quasars brighter than  $V=18.5$ , Einstein-de Sitter model.

The solution I propose also resembles that reached for the nebulae. There are two totally different types of object masquerading under the title of quasar. In one class the origin of the redshift is cosmological, the optical luminosities range from several to several hundred times that of a normal galaxy, and the radio luminosities and dimensions are in the range occupied by the strongest of the radio galaxies. In the second class the cosmological component of the redshift is close to zero and the objects are comparatively local, with some intrinsic redshift mechanism, perhaps gravitational, at work. The optical and radio luminosities are similar to those of radio-emitting Seyfert nuclei like NGC 1275 or NGC 1068.

A separation of quasars into two distinct classes suggests itself when the distribution of the quasars in depth is examined as a function of radio dimensions. The distribution in depth of a set of objects brighter than given optical and radio limiting flux levels can be examined by considering, for sources of a particular class (that is, with particular optical and radio luminosities and spectra), their distribution within the "observable volume" for this class of source<sup>10,11</sup>. If we imagine any particular quasar, which is visually brighter than 18.5 magnitudes and radio-wise brighter than the limit of the 3CR Survey<sup>12</sup>, say, moved to greater and greater distances (and redshifts), there will come a point, corresponding to  $z=z_{\max}$ , at which it would become fainter than either the optical or radio limiting flux levels. The corresponding (comoving) volume which is observable using this class of quasar is denoted by  $V_{\max}$ . If there are no evolutionary effects, that is, neither the luminosity nor the (comoving) number-density of the sources change with epoch (the proper number-density changes, of course, due to the expansion of the universe), then the number of objects of a particular class with redshift  $\leq z$  would, for  $0 \leq z \leq z_{\max}$ , simply be proportional to the comoving volume  $V(z)$ . Equivalently, if the quantity  $V/V_{\max}$  is calculated for each object, it should then be uniformly distributed between 0 and 1. This is true for each class of quasar, so it is true for the whole sample.

It has been found that the 3CR quasars are not uniformly distributed, in fact the mean value of  $V/V_{\max}$  is about 0.7 (ref. 10) (depending slightly on the cosmological model chosen for calculating  $V$ ), significantly different from the expected value for a uniform distribution of 0.5. This is interpreted as being due to evolutionary effects, either the frequency, power or

lifetime of the events giving rise to the quasar phenomenon, or the properties of some external medium with which the quasars interact, changing with epoch. It is naturally of interest to ask whether this evolution is related to the radio sizes of the quasars, since it has been suggested that different radio sizes correspond to different phases in the life of a quasar, compact quasars later evolving into quasars with extended radio sources<sup>13</sup>.

Fig. 1 shows  $V/V_{\max}$  against intrinsic radio size,  $d$ (kpc), for thirty-five quasars brighter than visual magnitude 18.5 identified with sources from the revised 3C catalogue<sup>13</sup>. Where the radio dimensions differ at different frequencies, the dimensions characteristic of the higher frequencies (where most of the energy is emitted) have been used, for example, for 3C 273, those of component B rather than component A (ref. 14).

There seems to be a rather abrupt change in the distribution of  $V/V_{\max}$  at around  $d=15$  kpc. For the larger sources the distribution is strongly non-uniform: none of the objects have  $V/V_{\max} < 0.5$ , and the mean value is 0.80. The smaller sources, on the other hand, have values of  $V/V_{\max}$  fairly uniformly distributed between 0 and 1, mean value 0.56.

At first sight this effect could be explained within the picture that the smaller quasars radio-wise evolve into the larger, by saying that the evolutionary effect implied by the non-uniform distribution of  $V/V_{\max}$  for the larger sources is due to the interaction of the radio-emitting components with some hypothetical intergalactic medium. But this type of explanation is inconsistent with the fact that the radio-quiet quasars also show a strong evolutionary effect<sup>15,16</sup>, which must therefore be due to some change with epoch in the population of the central (optical) objects. A similar result is obtained for radio-galaxies<sup>17</sup>.

An equally sharp division into two groups is obtained if apparent, rather than intrinsic, radio size is used. In this case the dividing line occurs at around  $\theta=3''$ .

The most surprising fact to emerge from this division into two classes is that all 7 3CR quasars belonging to Arp's proposed associations of radio-sources with peculiar galaxies<sup>6</sup> fall in the group with smaller radio dimensions. The statistical significance of Arp's associations has been challenged by several authors<sup>18-21</sup>: Arp has replied in ref. 22. The situation would seem to be that the association of 3CR sources with a particular section of Arp's catalogue of peculiar galaxies is significant if regarded in isolation, but may be thought of as a statistical fluctuation with respect to the catalogue as a whole. The probability that seven out of sixteen 3CR quasars with  $\theta < 3$  arc s (or  $d < 15$  kpc) should be in Arp associations (if the latter are pure chance), while none of the twenty-four with  $\theta > 3$  arc s are, is rather low  $(16/40)^7 \sim 0.0016$ .

The group of 3CR quasars with smaller radio dimensions contains eleven out of twelve objects with flat radio spectra, all three objects which are known to have varied violently in the optical<sup>23</sup> (3C 2, 345, 454.3), and all four which are known to have varied at radio frequencies<sup>24</sup> (3C 273, 345, 380, 454.3). When the same classification is applied to all quasars with known redshift and radio structure<sup>25</sup>, all six quasars with more than one absorption line, all four of the "1.95" objects<sup>7</sup>, and all eleven which have shown radio variations<sup>24</sup> belong to this group. Twelve out of thirteen quasars with  $z > 1.9$  belong to this group (and it is the large redshift quasars which are allegedly anisotropically distributed on the sky<sup>9</sup>).

I propose that this class of quasars consists primarily of comparatively local objects (10–100 Mpc); that the associations with Arp's peculiar galaxies are real, and that several other of these quasars may be associated with "near" Zwicky clusters<sup>26</sup>. A list of possible associations and estimated cosmological redshifts is given in Table 1. The radio and optical luminosities, as well as the radio dimensions and spectral indices, would then be in the same range as those of the more active Seyfert nuclei.

The group of quasars with larger radio dimensions, on the other hand, show a complete continuity with the stronger radio.



Table 1 Properties of "Local" Quasars

| 3C    | In association with Arp peculiar galaxy No. | 3C number of other member of association | Inside or just outside (*) boundary of Zwicky cluster No. | Adopted cosmological redshift | $\text{Log}_{10} P_V$ (W ster <sup>-1</sup> Hz <sup>-1</sup> ) | $\text{Log}_{10} P_{178}$ (W ster <sup>-1</sup> Hz <sup>-1</sup> ) |
|-------|---------------------------------------------|------------------------------------------|-----------------------------------------------------------|-------------------------------|----------------------------------------------------------------|--------------------------------------------------------------------|
| 2     | 35,201                                      | 15, 17 (G)                               |                                                           | ?                             |                                                                |                                                                    |
| 48    |                                             |                                          | 0107.5+3212 *                                             | 0.014                         | 19.4                                                           | 24.0                                                               |
| 138   |                                             |                                          | 0544.4+5036                                               | 0.03                          | 19.45                                                          | 24.8                                                               |
| 147   | 143                                         | 194                                      | 0800.0+0946                                               | 0.013                         | 18.75                                                          | 23.35                                                              |
| 186   |                                             |                                          | Virgo                                                     | 0.03                          | 19.25                                                          | 24.05                                                              |
| 191   | 134                                         | 274 (M87)                                |                                                           | 0.0038                        | 19.7                                                           | 23.0                                                               |
| 273   | 160                                         | 266 (G ?)                                |                                                           | 0.024                         | 18.25                                                          | 22.95                                                              |
| 277.1 |                                             |                                          | 1319.6+3.35 *                                             | 0.02                          | 19.0                                                           | 23.65                                                              |
| 286   | 139,196                                     | 277.3 (G)                                |                                                           | 0.038                         | 19.7                                                           | 24.3                                                               |
| 287   |                                             |                                          | 1416.0+0752 *                                             | 0.025                         | 19.55                                                          | 24.5                                                               |
| 298   |                                             |                                          |                                                           | ?                             |                                                                |                                                                    |
| 309.1 | 109                                         | 330                                      | 1625.5+4006 *                                             | 0.029                         | 20.2                                                           | 24.0                                                               |
| 345   | 125                                         | 337 (G)                                  | 1916.8+4855                                               | 0.006                         | 18.45                                                          | 23.35                                                              |
| 380   |                                             |                                          | 2256.5+1933 *                                             | 0.03                          | 19.25                                                          | 24.0                                                               |
| 454   |                                             |                                          | 2255.8+1350                                               | 0.025                         | 20.0                                                           | 24.0                                                               |
| 454.3 |                                             |                                          |                                                           |                               |                                                                |                                                                    |

galaxies if a cosmological interpretation of their redshifts is assumed. Moreover, Gunn's object, PKS 2251+11 (ref. 3), for which the redshift has been demonstrated to be the same as that of the nearby cluster, falls into this group. Thus I propose that these quasars are indeed "cosmological".

The proportion of 3CR sources which are "local" quasars would be only 5–10%. It should decline at first with decreasing flux density at 178 MHz, though it may become dominant at the very lowest flux levels. At high radio frequencies (above ~2,700 MHz), on the other hand, where a high proportion of the sources have flat radio spectra<sup>26</sup>, the contribution of "local" quasars may be dominant even at high flux densities, so that the flatter source-count slope<sup>28</sup> can easily be understood. On the other hand, the steep optical counts of radio-quiet quasi-stellar objects<sup>15</sup> suggest that the majority of these objects are not "local".

A detailed discussion of the arguments presented here, of some of the implications of the proposal that some quasars are "local" and some "cosmological", and of possible explanations of the intrinsic redshift component in the "local" quasars, will be given later.

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## Generation of Sedimentary Facies on a Spreading Ocean Ridge

THE way in which a spreading ridge system affects the pattern of pelagic sedimentation with time is not yet understood in detail. For example, what rules govern the geometry of facies boundaries on a spreading ridge system, especially if the boundaries fluctuate with climatic change? What are the rules for a migrating ridge; and for a ridge which has variable spreading rates? So far sedimentologists and stratigraphers have had to use a "layer-cake" framework because of the paucity of oceanographic data (with a few exceptions<sup>1,2</sup>). It seems certain, however, that sedimentation occurring with seafloor spreading will give rise to intricate facies relationships. Intricate relationships are well known to occur in sedimentary rocks and diachronism may prove to be the rule among pelagic sediments.

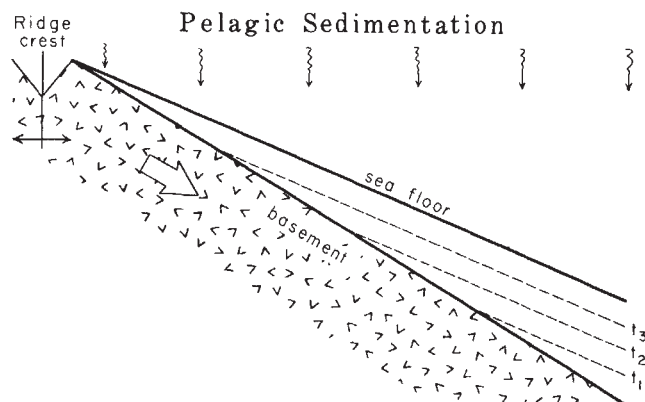


Fig. 1 Cross section through the right-hand half of an actively spreading ridge. Stippled area represents basaltic basement of the oceanic crust. Lines at  $t_1$ ,  $t_2$  and  $t_3$  correspond to time planes in the wedge of accumulating sediment

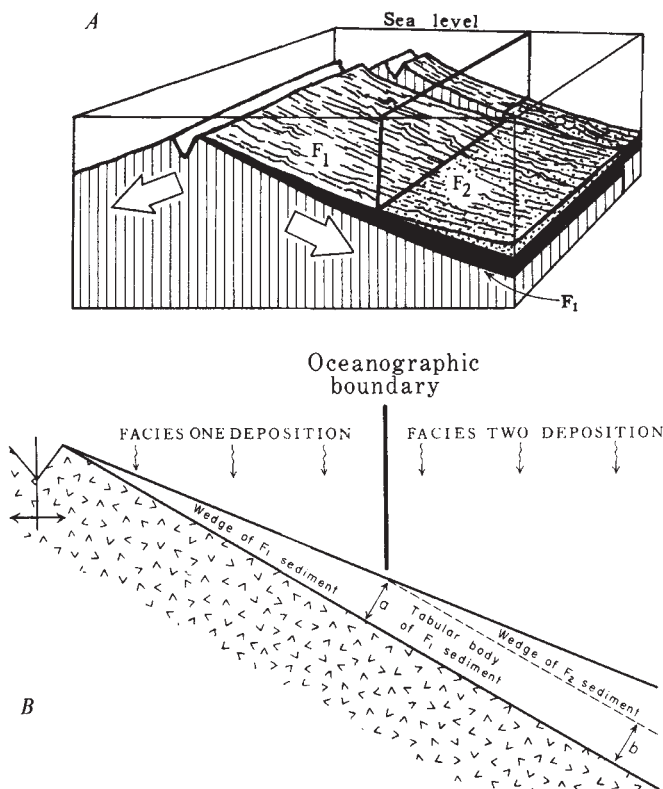


Fig. 2 A, Deposition on a spreading ridge system. An oceanographic boundary divides the ridge into two sedimentary processes,  $F_1$  and  $F_2$ . B, Details of facies geometry on a spreading ridge. Thickness of  $F_1$  sediments is constant to the right of the oceanographic boundary (as at "a" and "b").

The distribution of sediments in the deep ocean basins varies with hydrographic conditions such as water temperature, salinity, currents, and other variables such as calcium carbonate compensation level and distance from land. The variation of these factors with time gives rise to differences among sediments laid down at the same time. In this article we examine three-dimensional relationships among sedimentary facies that are simultaneously affected by seafloor spreading at varying rates, by migration of oceanographic boundaries due to climatic change or other causes, by variable sedimentation rates, and by migration of the entire ridge system. The purpose is to determine whether the shape and disposition of sedimentary units relative to their neighbours can be used to decipher problems in marine geology.

The wedge form of sediment deposited on one flank of a spreading ridge is shown in Fig. 1. The time planes represent past positions of the seafloor; in the ideal case they are parallel to the present seafloor and to each other. Successively younger time planes intersect the basaltic rock of the oceanic crust at positions progressively nearer the crest, as a result of the "conveyor-belt" nature of the spreading. In these circumstances stratification in the sediment will tend to be parallel to the time planes.

We now consider a simplified ridge, one flank of which is spreading through a stationary oceanographic boundary (Fig. 2), such as the Australia-Antarctic ridge spreading through the Polar Front or the East Pacific rise through the west margin of the Humboldt Current. Sediment type is different on either side of the barrier as a result of differing hydrographic conditions (for example, the diatomaceous ooze from cold surface waters south of the Polar Front contrasts with the foraminiferal ooze from relatively warm water north of it). The form of the two presently accumulating facies ( $F_1$  and  $F_2$ ) is wedge shaped. The facies will be separated by a boundary which is roughly parallel to the interface between basement and sediment, and which intersects the seafloor at the position of the oceanographic boundary. An important point is that north of the

Polar Front,  $F_1$  (= diatom ooze) is a tabular body which has been spread northward by motion of the floor away from the ridge crest. It was not deposited north of the front but reached its position there by seafloor spreading. The thickness of the tabular body is a function of the rate of accumulation of diatoms on the seafloor, the rate of spreading, and the distance of the oceanographic boundary from the ridge crest. For the tabular portion of  $F_1$ , the distance of the oceanographic barrier from the ridge crest at some time in the past can be computed as follows:

$$D = TV_s$$

where  $D$  is the distance,  $T$  is the time necessary for accumulation of the tabular body (dated directly, or by application of a sedimentation rate to a known thickness), and  $V_s$  is the spreading rate.

Sedimentation rates may vary on either side of the oceanographic barrier. The main effect will be on the degree of wedging (Fig. 3). Slow accumulation leads to relatively acute wedging and therefore steep slopes ( $F_1$  in Fig. 3A;  $F_2$  in Fig. 3B); fast accumulation produces less acute wedging and comparatively shallow slopes. The circumstances shown in Fig. 3A, where accumulation of  $F_1$  is relatively slow, lead to seafloor and time planes which are concave upward. In Fig. 3B, where  $F_1$  is accumulating more rapidly, they will tend to be convex upward, though these forms may be unrecognizable with the information generally available in an oceanographic study. In both cases, however,  $F_1$  will be expressed as a tabular body to the right of the barrier, the thickness of which will vary with sedimentation rate.

A change in the rate of spreading of the ridge system will lead to changes in shape of both  $F_1$  and  $F_2$ . With equal sedimentation rates on either side of the oceanographic barrier, a decrease in spreading rate at some time in the past will cause an increase in thickness of  $F_1$ , which has accumulated between the ridge crest and the oceanographic barrier (Fig. 4A). During the present, less rapid spreading, the tabular body of  $F_1$  is accumulating to a greater thickness because it is exposed to  $F_1$  sedimentation for a longer interval of time than it was under

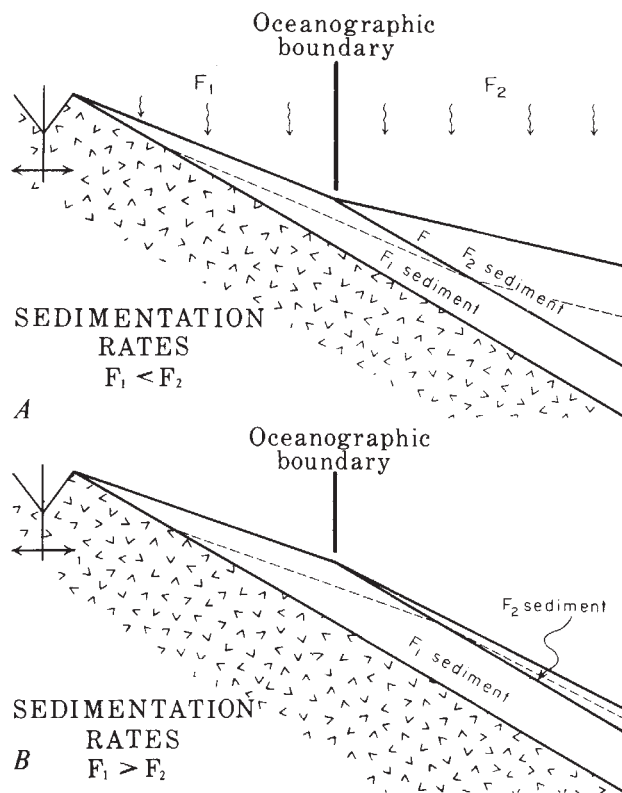


Fig. 3 A, Effect of sedimentation rate of  $F_1$  being less than that in  $F_2$ ; B, effect of sedimentation rate of  $F_1$  exceeding that of  $F_2$ . Dashed line shows a hypothetical time plane.



the earlier, more rapid spreading period. But the transition from a thin to a thick tabular body of  $F_1$  will not be immediate. The full effect of the rate change on sediment accumulation will not be evident until the entire length of the pre-existing wedge of  $F_1$  has passed the oceanographic boundary. A tapered block of sediment thus connects the two tabular bodies; length of the taper is equal to the distance from crest to barrier. For  $F_1$  decreasing spreading rate tends to produce upward convexity in the seafloor and in time planes within the unit, similar to Fig. 3B, where the effect is produced by the sedimentation rate of  $F_1$  exceeding that of  $F_2$ .

Results of an increase in spreading rate are shown in Fig. 4B; the taper is simply reversed from the case of decreasing spreading rates. Note also that increasing spreading rate is analogous to the situation in Fig. 3A (sedimentation of  $F_2$  is more rapid than  $F_1$ ), in that the seafloor tends toward upward concavity. Ewing and Ewing<sup>3</sup> recognized such steep upward concavity on several ridge systems, and chose to interpret these profiles as being due to discontinuities in spreading rates.

A migrating or fluctuating oceanographic boundary presents an interesting problem on a spreading ridge system. Such a moving boundary might correspond, for example, to a diatom-foram separation as before, or it might represent the intersection of the calcium carbonate compensation level with the sloping ridge flank. In either case, significant palaeoclimatological or palaeo-oceanographic variations might be detectable by analysis of the facies geometry. Two cases are shown in Fig. 5—cases A and B show the relationships between  $F_1$  and  $F_2$  where the oceanographic boundary is migrating in the direction of spreading. In A, migration rate exceeds spreading rate; and in B, the opposite is true. The left side of Fig. 6B shows the effects of migration of the barrier in an opposite

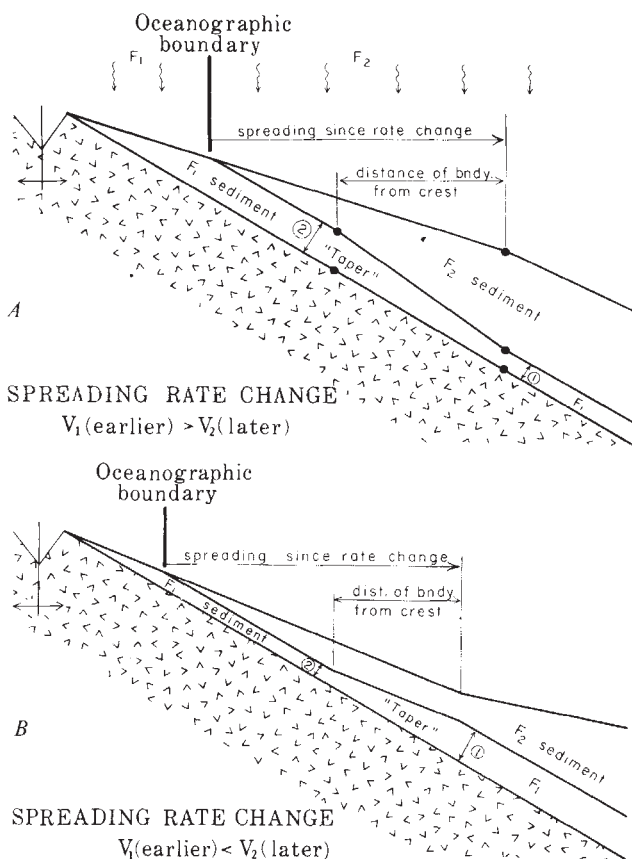


Fig. 4 Effect of changing rate of seafloor spreading at some time in the past. Distance of boundary from crest is reflected in length of tapered segment of  $F_1$ . Amount of spreading since rate change is equal to the distance from the present position of the oceanographic barrier to the break in slope in  $F_2$ . Thickness "one" accumulated under earlier spreading rate, "two" under more recent one. A, Earlier rate exceeds later one; B, earlier rate less than later one.

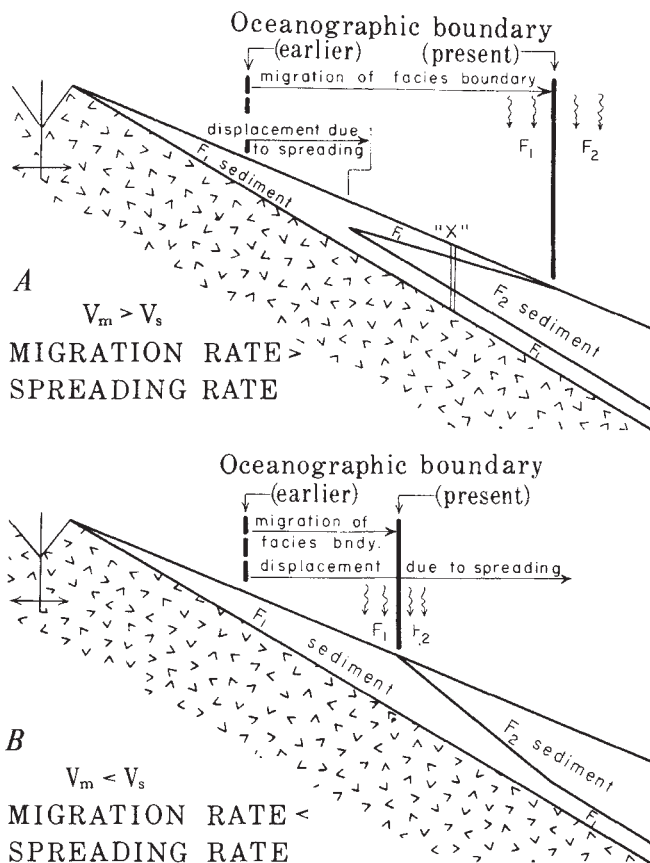


Fig. 5 Effect of migration of oceanographic boundary. A, Migration rate exceeds spreading rate (hypothetical core at point "X"); B, migration rate less than spreading rate.

sense to that of the spreading. Clearly the resulting facies geometries in all cases will be the same as for a stationary oceanographic boundary and a migrating ridge system in motion in the opposite sense as indicated for the barrier. This means that it is not possible to determine, without independent evidence, whether facies geometries arise from migrating oceanographic boundaries or from migration of the entire ridge system.

In Fig. 5,  $F_2$  always overlies  $F_1$ , except in the case of the upper part of Fig. 5A, where the reverse is true. The dip of the facies boundary is dependent on the value of the spreading rate relative to the migration rate. For spreading and migration in the same direction, equal spreading and migration rates yield a vertical contact between  $F_1$  and  $F_2$ . The apparently anomalous situation of  $F_1$  overlying  $F_2$  in case 5A results from the fact that  $F_1$  is transgressing over territory formerly undergoing  $F_2$  deposition, due to the rapid motion of the barrier. Note also that a core at point "X" (Fig. 5A), by penetrating the lower plate of  $F_1$  (which has reached its present position solely by spreading), would falsely suggest a crestward migration of  $F_2$ . Palaeoclimatological and palaeo-oceanographic conclusions must be carefully stated in such a situation.

Generally, the vertical sequence may reveal details of the climatological or oceanographic history of the ridge. There are, however, exceptions. In the simple situation of Fig. 2, one might conclude that  $F_2$  has physically transgressed over  $F_1$ , whereas the geometry actually results from  $F_1$  being moved post-depositionally by spreading onto the realm of  $F_2$ . Further, in Fig. 5B, spreading at a high rate has resulted in similar displacement of  $F_1$  to the extent that the oceanographic boundary appears to have migrated in a sense which is opposite to its actual motion. In the case of the Polar Front example, this would lead to the erroneous conclusion that the diatom-foram barrier has migrated southward rather than northward. To arrive at the correct interpretation, it is necessary to know only the spreading rate and the sedimentation rates.

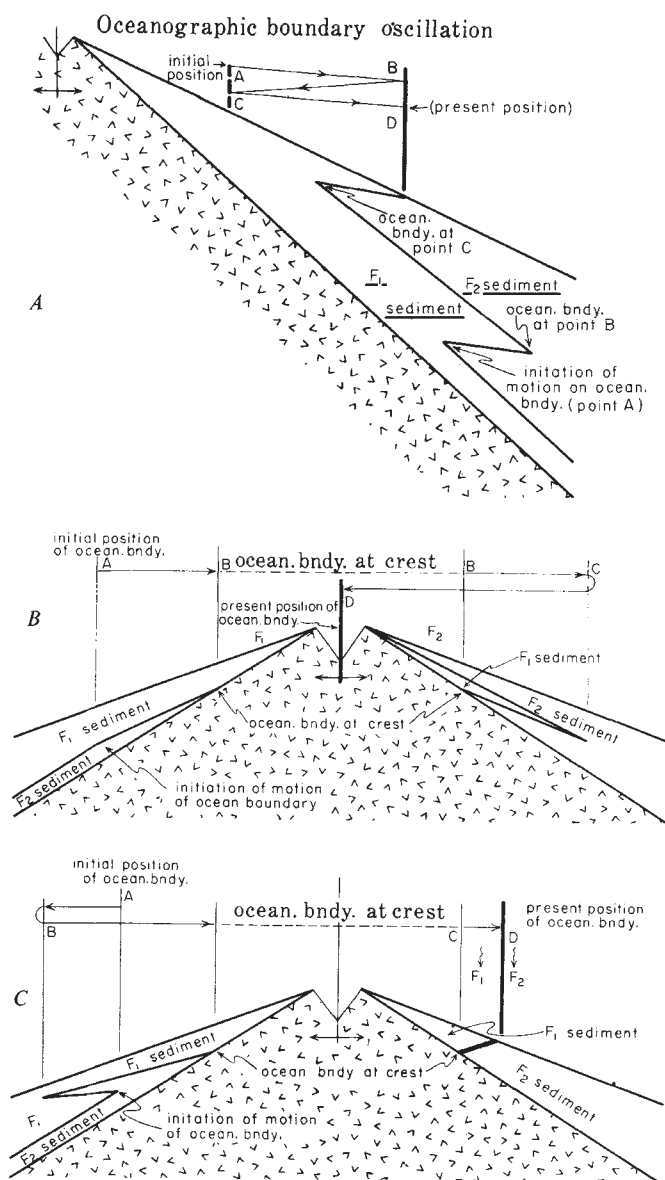


Fig. 6 Effect of oscillation of oceanographic boundary. A, Oscillation on one flank as shown by arrows; B and C, oscillation across the ridge crest, as shown by arrows. Dashed central section reflects passage of boundary across the crest and subsequent spreading.

Other examples are shown in Fig. 6. Tongues of sediment are produced in response to reversals in direction of migration; these lie on opposite flanks if migration is across the ridge crest. Such a situation may have been in effect in the past on the Australia-Antarctica ridge<sup>4</sup>; at present the Polar Front coincides closely with the ridge crest. In all these examples, the possibility of a faulty interpretation of migration direction results only from the spreading of the basal tabular body of one facies into the depositional regime of the second facies.

The conditions analysed above may be operative, singly or as a group, on any spreading ridge system. Their cumulative effects on the geometry of sedimentary facies may tend to accentuate or to obscure the relationships discussed here. Moreover, hydrographic factors which can cause sediment variations seem to be characterized by small fluctuations, and this will lead to less obvious, though real, changes in geometry. Most facies boundaries shown here probably would be saw-toothed on a smaller scale due to such fluctuations. But the gross geometric relationships of pelagic facies on a ridge system must be interpreted in the light of the spreading process itself before meaningful conclusions can be drawn about palaeoclimatology and palaeo-oceanography.

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## BIOLOGICAL SCIENCES

### Sex Difference in Acetaldehyde Exhalation following Ethanol Administration in C57BL Mice

ACETALDEHYDE (AcH) is the intermediary compound in the metabolic conversion of ethanol to acetate. Because AcH is a potent pharmacological agent in its own right, it has been the object of much recent investigational interest<sup>1,2</sup>. Many of the unpleasant symptoms observed in the alcohol-antabuse reaction have been attributed to acute increase of blood AcH<sup>3</sup>. It was suggested that barbiturate-induced increases in liver AcH dehydrogenase activity might contribute to the ethanol tolerance seen in persons who use barbiturates regularly<sup>4</sup>. Other properties of AcH, such as the release of catecholamines<sup>1</sup> or the condensation with catecholamines to form tetrahydro-isoquinoline alkaloids<sup>5,6</sup>, may contribute to either the subjectively pleasant or unpleasant effects of alcoholic beverages.

Here we report a substantial sex difference in AcH after a standard intraperitoneal dose of ethanol. Acetaldehyde exhalation by male C57BL/6J mice was several times greater on the average than that by females of the same strain. Castration diminished the response of male mice to the female range. Ovariectomy had no effect.

Acetaldehyde and ethanol in expired air were measured by an adaptation of the gas chromatographic method established by Freund and O'Hollaren<sup>7</sup> for studying human alveolar air. This has the advantage that a large number of serial samples can be obtained readily from one animal, and analysed immediately, without prior purification, by direct injection into the gas chromatograph. Male and female C57BL/6J mice averaging 20 g were obtained from Jackson Laboratories, Bar Harbor, Maine. Castration and ovariectomy were performed with standard techniques under ether anaesthesia by the Mason Research Foundation, Worcester, Massachusetts. Each animal received an intraperitoneal injection of ethanol (4 g kg<sup>-1</sup>, 25% v/v solution in isotonic saline). After 15 min the mouse was placed in a glass chamber of approximately 170 ml. capacity (50 mm high and 70 mm diameter). The chamber was flushed and filled with oxygen and the top was sealed with 'Parafilm' (Will Scientific, NY) held in place with a rubber band. Gas samples were withdrawn from the chamber through a small plastic catheter into a 1 ml. plastic syringe mounted on a three-way metal stopcock. Before withdrawal of a sample, the tubing was flushed and the chamber atmosphere was thoroughly mixed by drawing back and forth on a 10 ml. syringe which



was similarly mounted on the stopcock. Dimpling the 'Parafilm' allowed this to be done without breaking the seal. The entire sample (1 ml.) was immediately injected into a Hewlett-Packard model 5750 gas chromatograph which was equipped with a column (1/8 inch by 6 feet) packed with 50-80 mesh 'Poropak N' (Applied Science Laboratories, State College, Pennsylvania). The column temperature was 160° C and the carrier gas flow (helium zero gas, Matheson) was 30 ml. min.<sup>-1</sup>. Instrumental settings were: AcH, range 1, attenuation 2; and ethanol, range 10<sup>2</sup>, attenuation 4. Retention times were 2.2 min for AcH and 4.4 min for ethanol. AcH and ethanol concentrations were estimated from peak heights in comparison with gas standards<sup>7</sup>. Duplicate samples of gas standards agreed to within 5%. The minimum amount of AcH that could be detected was 0.1 ng.

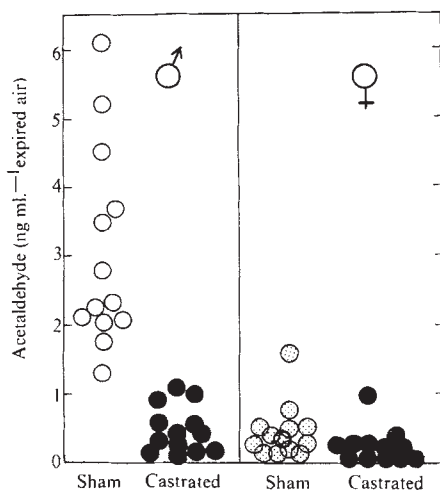


Fig. 1 Acetaldehyde levels in expired air for individual sham operated and castrated male and female C57BL/6J mice. Analyses were performed 9 weeks after surgery. Animals were individually placed into the rebreathing chamber 15 min after the ethanol injection (4 g kg<sup>-1</sup>). The chamber atmosphere was sampled 17 min later (32 min after injection). The difference in mean AcH levels (ng ml.<sup>-1</sup> ± s.d.) for the male groups was statistically significant (Student's *t* test, one-tailed): castrated 0.47 ± 0.32 (*N* = 13) and sham operated 3.07 ± 1.41 (*N* = 13), *P* < 0.001. The difference in mean AcH levels in the female groups was not statistically significant: ovariectomized 0.23 ± 0.22 (*N* = 12) and sham operated 0.42 ± 0.45 (*N* = 13), *P* > 0.2. Differences in mean ethanol levels (μg ml.<sup>-1</sup> ± s.d.) were not significant: males, castrated 2.20 ± 0.24 (*N* = 13) and sham operated 2.27 ± 0.37 (*N* = 13), *P* > 0.9; females, ovariectomized 2.34 ± 0.28 (*N* = 12) and sham operated 2.31 ± 0.28 (*N* = 13), *P* = 0.8. Results similar to those above were obtained for analyses of samples obtained at 11 min.

In an initial study, samples were obtained and analysed 5 min after placing each animal into the chamber and at 6 min intervals thereafter. Results with ten male and ten female mice are shown in the table. The mean AcH levels for male mice are about five times higher than the means for females (*P* < 0.001 for all time periods, Student's *t* test, two-tailed). Although mean AcH levels were relatively stable with time, there were in individual animals gradual changes in either an upward or a downward direction and cyclical changes were also apparent. Previously, Ridge<sup>8</sup> had observed cyclical changes of AcH in the blood and brain of rabbits. Ethanol levels were maximal at 17 min and declined slowly thereafter. Differences in mean ethanol levels between males and females were not statistically significant at any time period (*P* > 0.5). Seventeen min (that is, 32 min after injection) was selected as a representative time period for later studies.

In subsequent studies, castrated mice of both sexes were compared with sham operated controls. Results at 9 weeks after surgery (Fig. 1) showed that castration had lowered the mean AcH levels (see legend to Fig. 1) in the males by 85% (*P* <

0.001, Student *t* test, one-tailed). The AcH levels in the females were not significantly different. Differences in ethanol levels between castrated and sham operated animals were not significant (see legend).

**Table 1** Mean Acetaldehyde and Ethanol Levels in Expired Air for Groups of Ten Male and Ten Female C57BL/6J Mice after Intraperitoneal Injection of Ethanol (4 g kg<sup>-1</sup>)

| Time (min) | Females ( <i>N</i> = 10)                   | Males ( <i>N</i> = 10) |
|------------|--------------------------------------------|------------------------|
|            | Acetaldehyde (ng ml. <sup>-1</sup> ± s.d.) |                        |
| 5          | 0.54 ± 0.46                                | 3.18 ± 0.42            |
| 11         | 0.57 ± 0.71                                | 3.27 ± 0.75            |
| 17         | 0.61 ± 0.88                                | 2.96 ± 0.66            |
| 23         | 0.73 ± 1.15                                | 3.32 ± 1.34            |
| 29         | 0.74 ± 1.12                                | 3.69 ± 1.80            |
| 35         | 0.79 ± 1.02                                | 4.17 ± 2.36            |
|            | Ethanol (μg ml. <sup>-1</sup> ± s.d.)      |                        |
| 5          | 1.53 ± 0.30                                | 1.61 ± 0.25            |
| 11         | 2.53 ± 0.44                                | 2.60 ± 0.46            |
| 17         | 2.69 ± 0.39                                | 2.71 ± 0.38            |
| 23         | 2.59 ± 0.34                                | 2.61 ± 0.33            |
| 29         | 2.43 ± 0.32                                | 2.41 ± 0.32            |
| 35         | 2.27 ± 0.30                                | 2.20 ± 0.31            |

Animals were individually placed into the rebreathing chamber at 15 min after the ethanol injection. The chamber atmosphere was sampled between 5-35 min later (20-55 min after injection) and analysed by gas chromatography. The differences in AcH levels between males and females were significant at all time periods (*P* < 0.001). Differences in ethanol levels were not significant at any of the time periods (*P* > 0.5).

The same animals had been examined 2-5 weeks earlier, when a large and statistically significant difference between the two male groups was evident while the female groups again were not significantly different, but there was greater variability within the groups. Two weeks after surgery, the mean AcH levels (ng ml.<sup>-1</sup> ± s.d.) in expired air of the male groups at 17 min were: castrated 1.33 ± 1.24 (*N* = 15) and sham operated 3.51 ± 2.39 (*N* = 15), *P* < 0.01. Five weeks after surgery, the mean AcH levels at 17 min in randomly selected females from the two groups were: ovariectomized 1.46 ± 1.53 (*N* = 10), and sham operated 1.31 ± 0.88 (*N* = 10), *P* > 0.7. For each animal in the male and female groups at both 2 and 5 weeks after surgery, a second chamber atmosphere sample was obtained at 11 min; results were similar to those at 17 min.

These results probably reflect the effects of testicular hormones acting on tissue enzymes. The difference between sexes could be due either to relatively increased production or slower removal of AcH in males, or both factors may be involved. Sex differences in the drug-metabolizing enzymes of the liver endoplasmic reticulum of rats have been reported<sup>9-11</sup>. Of particular interest is a microsomal ethanol-oxidizing system which has many characteristics of the drug-metabolizing enzymes<sup>12</sup>; male rats exhibit greater activity<sup>13</sup>. Perhaps this system plays a critical role in AcH production. Another possibility is the enzyme catalase, which can oxidize ethanol to AcH in the presence of hydrogen peroxide<sup>14</sup>. Adams<sup>15</sup> showed that male albino mice had substantially higher measurable levels of liver catalase than females; castration lowered the male levels but did not affect the females. These observations are similar to our observations with AcH in expired air. The C57BL mice may be an exception, however, for no sex difference in catalase was observed for this strain<sup>16</sup>; this point bears rechecking. Still other possibilities are a steroid-dependent aldehyde dehydrogenase in liver<sup>17</sup> and a testosterone-dependent alcohol dehydrogenase in kidney<sup>18</sup>. These possibilities and others remain to be explored.

The sex difference in AcH in C57BL mice has implications for investigations concerned with the role of AcH in the

pharmacological responses to ethanol. The C57BL mice are popular for alcohol studies because this is the strain that spontaneously prefers to drink solutions of 10% ethanol. An attempt was made recently<sup>19</sup> to relate differences in drinking preference between female mice of the C57BL/6J and DBA/2J strains to differences in AcH. In a later investigation<sup>2</sup>, the sex of the mice was not specified. Further investigations of male-female differences within the same strain may provide valuable information. Furthermore, differences in drinking practices between men and women in our society are established<sup>20</sup> and have usually been attributed to social causes. The metabolism of AcH in men and women, however, has not been compared. Such studies seem to be warranted.

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## Human Growth Hormone and Ketosis in Athletes and Non-athletes

WHEN the blood levels of ketones and other metabolites were studied in athletes and non-athletes during and after running for 1.5 h, the subjects in athletic training showed hardly any post-exercise ketosis whereas the untrained group had a considerable rise of ketones after exercise, even though their level of activity was much less<sup>1</sup>. The difference may be related to the hormonal regulation of metabolism during exercise. Growth hormone secretion is known to occur during exercise<sup>2</sup> and subjects in regular athletic training have a smaller rise compared with non-athletes<sup>3</sup>. We have investigated the relationship between ketosis and growth hormone secretion.

Five subjects, three males and two females (aged 19–50 yr), were subjected to an acetoacetate tolerance test (Fig. 1). The

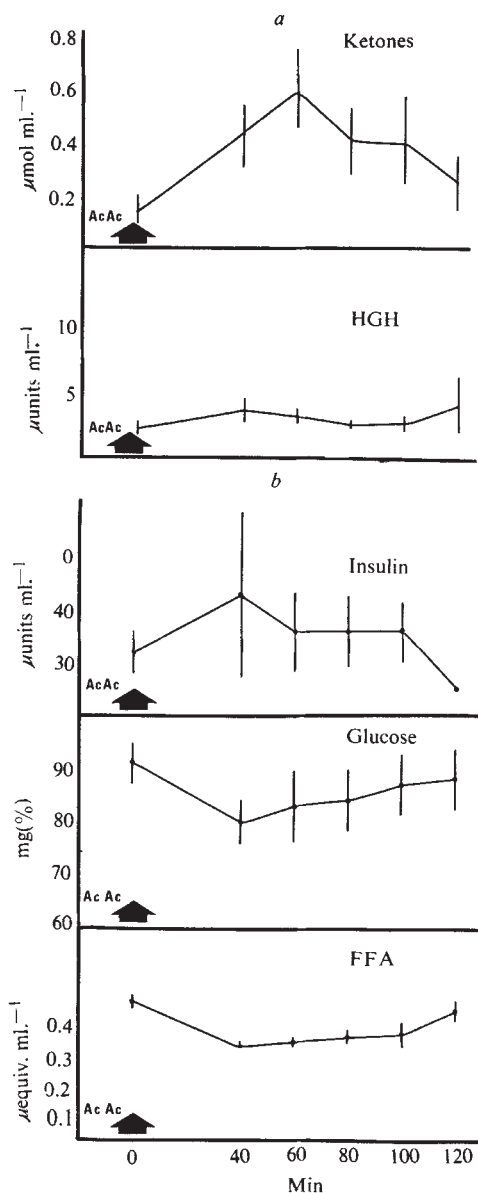


Fig. 1 The changes (means  $\pm$  s.e.m.) in the five subjects of (a) total ketone bodies (acetoacetate + 3-hydroxybutyrate) and growth hormone and (b) insulin, glucose and free fatty acids (FFA) after ingestion of a solution of sodium acetoacetate (200 ml., 0.4 M) at the time indicated.

subjects had fasted for at least 8 h and remained at rest throughout the investigation, which was carried out between 0800 h and 1200 h. A venous blood sample (20 ml.) was taken before ingestion of sodium acetoacetate (200 ml., 0.4 M)<sup>4</sup>, 40 min later and then at 20 min intervals for a further 80 min. Blood samples were analysed by enzymic methods for acetoacetate, 3-hydroxybutyrate<sup>5</sup> and glucose<sup>6</sup>. A colorimetric method was used for free fatty acids (FFA)<sup>7</sup> and radioimmunoassay techniques for plasma insulin<sup>8</sup> and growth hormone<sup>9</sup>. The human growth hormone standard used was the International Reference Preparation.

The differences in growth hormone release<sup>3</sup> and ketone production<sup>1</sup> in athletes compared with non-athletes suggest that there is a relationship between growth hormone secretion and ketosis. Observations supporting this possibility have recently been obtained<sup>10</sup>. Some metabolic substrates such as amino-acids<sup>11</sup> may initiate secretion of growth hormone and it could be argued that the ketosis causes the increase in plasma growth hormone with exercise. The degrees of ketosis obtained in this present investigation are within the range which may be observed in exercise; nevertheless no significant change



in growth hormone occurred even though there were also changes in glucose, insulin and FFA as a result of the ketone ingestion. These observations indicate that the degree of post-exercise ketosis may depend on differences in growth hormone secretion rather than vice versa.

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## Effect of Prostaglandin $F_{2\alpha}$ on the Early Pregnancy of Rabbits

SUBCUTANEOUS injection of prostaglandin  $F_{2\alpha}$  (PGF $_{2\alpha}$ ) prevents pregnancy in hamsters<sup>1-3</sup> and blocks nidation in the rat and rabbit<sup>4</sup>. Subcutaneous injection or intravenous infusion of prostaglandin  $E_2$  or  $F_2$  terminates pregnancy in the rhesus monkey<sup>5</sup>. Therapeutic abortion using prostaglandin in medical practice has also been reported<sup>6-10</sup>. The following experiment was performed in an attempt to reveal the mechanism of action of prostaglandin.

Adult rabbits of mixed breeds weighing from 1.5 (Dutch belts) to 4.1 kg (New Zealand white), which had been caged individually for 3 weeks, were artificially inseminated with pooled semen collected from fertile bucks and intravenously injected with human chorionic gonadotrophin (HCG) for the induction of ovulation. They were injected subcutaneously on various days after insemination with PGF $_{2\alpha}$  (glassy material, pure compound, or methoxamine salt). In most cases, the dose, based on body weight, was given as two equal injections in the early morning and late afternoon. In two groups of rabbits PGF $_{2\alpha}$ , placed in a 'Silastic' tube (601-321 tube, sealed with medical adhesive, Dow Corning) was implanted under the skin on day 3 after insemination. The rabbits were killed in most cases from day 9 to day 14 after insemination and the number of corpora lutea, uterine swellings (implantation sites), maternal placentae (deciduoma), foetal placentae with or without degenerated embryos, and living foetuses were counted.

The total number of corpora lutea, the proportion of implantation sites and the living young in each group were compared for statistical purposes with those groups in which no effect was observed<sup>11</sup>. In the absence of uterine swellings, the uterus was flushed with saline for the recovery of eggs. A few of the ovaries were fixed, sectioned and stained with haematoxylin and eosin for histological examination.

The following points can be made from the results presented in Table 1. (1) Subcutaneous injection of 0.5 to 2 mg PGF $_{2\alpha}$ /kg/day on days 3, 4 and 5 (groups I, II and IV) or implantation of PGF $_{2\alpha}$  in a 'Silastic' tube of 5 to 20 mg/rabbit on day 3 (groups XIV and XV) had no effect on implantation (60-88%) and the development of embryos (54-83%): the proportions of implantation sites and living embryos to the number of corpora lutea in each group are very similar to those obtained previously in our laboratory. (2) Subcutaneous injection of a dose of 1 or 2 mg/kg/day on days 3, 4, 5, 6 and 7 (groups III and VII) did not disturb implantation (71-79%), but significantly inhibited ( $P < 0.05$ ) the development of embryos (42-45%). (3) Injection of 2 mg/kg/day on days 8, 9 and 10 (group V) soon after implantation, however, caused complete degeneration of embryos (0%); a dose of 5 mg/kg/day injected on days 3, 4, 5, 6 and 7 (group VIII) reduced the number of corpora lutea and inhibited implantation significantly (15%) and development of embryos completely (0%). (4) When PGF $_{2\alpha}$  at a dose of 5 mg/kg was injected either on day 1 or 2 (groups X and XI), implantation (17-31%) was significantly disturbed ( $P < 0.001$ ), but embryonic development after implantation (12-31%) was not significantly affected; administration either on day 4 or 6 (groups XII and XIII), however, had little effect either on implantation (53-78%) or on embryonic development (47-66%).

In a final experiment (groups VI and IX), PGF $_{2\alpha}$  was injected subcutaneously 21 days after insemination. When given 2 mg/kg/day, three rabbits aborted living foetuses and placenta 2 days after treatment. When given 5 mg/kg/day, all four rabbits aborted on the second or third day after treatment.

Because subcutaneous injection of 5 mg/kg of PGF $_{2\alpha}$  on day 1 or 2 after insemination prevented implantation, rabbits were injected with 0.5, 1, 2 or 5 mg/kg of PGF $_{2\alpha}$  on day 1 and examined on day 2. Table 2 shows that no egg was found in any part of the female tract when the dose was 5 mg/kg. With 0.5-2 mg/kg, little disturbance of egg transport was observed and most eggs were fertilized and normal, although two were recovered from the uterus and vagina. It seems therefore that PGF $_{2\alpha}$  affects the Fallopian tubes and uterine horns so that all or most of the eggs are expelled from the female tract.

Histological examination of ovaries of a few rabbits treated with 1-5 mg/kg/day of PGF $_{2\alpha}$  for 3 to 5 days has shown that many corpora lutea are not distinctive and smaller; only in a few corpora lutea were degenerative changes of luteal cells seen.

PGF $_{2\alpha}$  seems to be quite toxic. Five of thirty-nine rabbits (13%) died after the first injection of 2.5 mg/kg. One of twenty-three rabbits (4.4%) died after the first injection of 1 mg/kg of PGF $_{2\alpha}$ . A subcutaneous injection of 0.25 or 0.5 mg/kg or implantation of prostaglandin in 'Silastic' tube of 5 or 20 mg/rabbit did not cause any death among thirty-three rabbits. Death occurred soon after injection; one animal was found dead 5 min after the injection and another within 30 min; the others were found dead 2 to 6 h after injection. At autopsy, blood patches were found around the capillary blood vessels in the intestine and in the uterus in a few animals, but no other obvious symptoms could be determined. The surviving animals all appeared to be healthy and their body weight was fairly constant, only a slight increase or decrease being observed between injection and autopsy.

Prostaglandin is known to stimulate the contraction of the uterus and the Fallopian tubes *in vitro* and *in vivo*<sup>12</sup>. In the live rabbit PGF $_{2\alpha}$  causes contraction of the Fallopian tube<sup>13</sup> whereas PGEs reduce motility and relax the isthmus circular muscle and reduce opening pressure<sup>14</sup>. Induction of luteolysis

**Table 1** Effect of Prostaglandin  $F_{2\alpha}$  on the Early Pregnancy of Rabbits

| Groups | Dose (mg/day) * | Day of treatment | No. of females † | Total No. of corpora lutea | Total implantations + (%) ‡ | Degenerating blastocysts | Maternal placenta | Foetal placenta | Living embryos + (%) ‡ |
|--------|-----------------|------------------|------------------|----------------------------|-----------------------------|--------------------------|-------------------|-----------------|------------------------|
| I      | 0.5/kg          | 3, 4, 5          | 6 (6)            | 64                         | 56 (88)                     | —                        | 1                 | 2               | 53 (83)                |
| II     | 1/kg            | 3, 4, 5          | 6 (5)            | 61                         | 42 (69)                     | 0                        | 1                 | 2               | 39 (64)                |
| III    | 1/kg            | 3, 4, 5, 6, 7    | 6 (3)            | 51                         | 36 (71)                     | 0                        | 7                 | 6               | 23 (45)                |
| IV     | 2/kg            | 3, 4, 5          | 6 (6)            | 70                         | 53 (76)                     | —                        | 8                 | 7               | 38 (54)                |
| V      | 2/kg            | 8, 9, 10         | 6 (6)            | 51                         | 33 (65)                     | 0                        | 20                | 13              | 0 (0)                  |
| VI     | 2/kg            | 21, 22, 23       | 3                |                            |                             | Aborted                  |                   |                 |                        |
| VII    | 2/kg            | 3, 4, 5, 6, 7    | 4 (2)            | 38                         | 30 (79)                     | 0                        | 11                | 3               | 16 (42)                |
| VIII   | 5/kg            | 3, 4, 5, 6, 7    | 7 (1)            | 13                         | 2 (15)                      | 0                        | 2                 | 0               | 0 (0)                  |
| IX     | 5/kg            | 21, 22, 23       | 4                |                            |                             | Aborted                  |                   |                 |                        |
| X      | 5/kg            | 1                | 6 (4)            | 49                         | 15 (31)                     | 0                        | 0                 | 0               | 15 (31)                |
| XI     | 5/kg            | 2                | 6 (4)            | 76                         | 13 (17)                     | 3                        | 2                 | 2               | 9 (12)                 |
| XII    | 5/kg            | 4                | 3 (3)            | 34                         | 18 (53)                     | 1                        | 1                 | 1               | 16 (47)                |
| XIII   | 5/kg            | 6                | 3 (3)            | 32                         | 25 (78)                     | 2                        | 3                 | 1               | 21 (66)                |
| XIV    | 5/rabbit§       | 3                | 5 (5)            | 57                         | 38 (67)                     | 0                        | 0                 | 0               | 38 (67)                |
| XV     | 20/rabbit§      | 3                | 6 (6)            | 58                         | 35 (60)                     | 2                        | 0                 | 0               | 35 (60)                |

\* Subcutaneous injection twice daily.

† No. of females having implantation sites is in brackets.

‡ Based on the total number of corpora lutea.

§ 'Silastic' tube containing prostaglandin was implanted under the skin.

by administration of prostaglandin has also been reported<sup>15</sup>. From the results of this study, it seems that subcutaneous injection of  $PGF_{2\alpha}$  at a total dose of 5 mg/kg/day, soon after ovulation for 1 day, causes the disappearance of eggs from the tube and the uterus, probably by stimulation of the contractility of these organs. Administration of  $PGF_{2\alpha}$  at the same dose for 5 days when the eggs are in the uterus disturbs the development of the corpora lutea as well as that of the embryo and the normal process of implantation. Administration of 2 mg/kg for 3 days after implantation causes complete degeneration of embryos, which is probably due to some luteolytic effect. Abortion can also be induced by a dose of 2 to 5 mg/kg/day when subcutaneously injected 21 days after insemination. The effective dose, however, is close to a lethal dosage for the rabbit.

**Table 2** Subcutaneous Injection of Prostaglandin  $F_{2\alpha}$  One Day after Insemination on the Recovery of Rabbit Eggs on Day 2

| Dose (mg/day) | No. of rabbits | Total corpora lutea | Total       | No. of eggs recovered |             |             |
|---------------|----------------|---------------------|-------------|-----------------------|-------------|-------------|
|               |                |                     |             | from tubes            | from uteri  | from vagina |
| 5             | 4              | 36                  | 0<br>(0%)   | 0                     | 0           | 0           |
| 0.5-2         | 6              | 56                  | 44<br>(79%) | 42<br>(96%)           | 1<br>(2.3%) | 1<br>(2.3%) |

For therapeutic abortion, infusion of 50  $\mu$ g/1.1in of  $PGF_{2\alpha}$  for an average of 14.5 h, a total dose of about 47 mg, or vaginal administration of 50 mg  $PGF_{2\alpha}$  every 2.5 h for an average of 14.5 h, a total of about 300 mg<sup>16</sup>, is required, but slightly higher doses have been reported for intravenous infusion or intrauterine administration of  $PGF_{2\alpha}$ <sup>9</sup>. Although intrauterine administration requires much less prostaglandin than intravenous infusion, a 40% failure by intrauterine administration was reported<sup>17</sup>. From this account and in consideration of our results on rabbits, even if human subjects are less sensitive to the toxic effect of  $PGF_{2\alpha}$  than the rabbit, the effect of  $PGF_{2\alpha}$  as an abortifacient and as an antifertility compound requires further investigation.

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## Aggressive Strain of *Ceratocystis ulmi* in Britain

DUTCH elm disease has been present in Great Britain for more than fifty years, but apart from heavy attacks in the 1930s it has generally been an endemic problem of no great importance, twig and branch infection being frequent but trees rarely dying<sup>1</sup>.

In the past few years, however, there has been a great increase



in the disease in parts of southern England. An assessment of the situation in 1971 is provided by a Forestry Commission ground survey, the full results of which will be published shortly. In an area of 56,600 sq km of southern Britain, including most of England south of a line from Birmingham to Norwich, it is estimated that there are 18 million elms, of which 4% are dying or dead and a further 9% have slight to moderate infection. The disease is regional in effect and the numbers of dying and recently dead trees in the eight worst affected counties are Essex, 109,000; Gloucestershire, 98,000; Worcestershire, 77,000; Kent, 66,000; Hampshire, 47,000; Suffolk, 42,000; Herefordshire, 26,000; and Sussex, 21,000.

In an investigation of this increased severity, isolates of *Ceratocystis ulmi* were made from infected trees in the outbreak regions. Another group of isolates was obtained from areas where the disease was at its endemic level (non-outbreak isolates).

In June 1971, five isolates in each group were tested for pathogenicity on young seedling elms at Alice Holt. In addition two of the outbreak isolates were used to inoculate a range of elm clones at Wageningen. The technique of inoculation and disease assessment were those employed in the elm breeding programme in Holland<sup>2,3</sup>. In brief, a spore suspension was prepared for each isolate by shaking pieces of mycelium in nutrient medium for 2 days, and this was injected into an incision made in the tree stem to expose wood vessels. Subsequent infection and dieback were expressed in terms of a disease index based on a scale from 0 = healthy to 4 = dead.

**Table 1** Effect of English "Outbreak" and "Non-outbreak" *C. ulmi* Isolates on Two Elm Species

| Origin of <i>C. ulmi</i> isolates | <i>U. americana</i>                     |                                         | <i>U. glabra</i>                                  |  |
|-----------------------------------|-----------------------------------------|-----------------------------------------|---------------------------------------------------|--|
|                                   | Disease index 6 weeks after inoculation | Disease index 6 weeks after inoculation | Percentage stem dieback 8 weeks after inoculation |  |
| <b>Outbreak</b>                   |                                         |                                         |                                                   |  |
| (1) Essex                         | 2.6                                     | 3.4                                     | 93                                                |  |
| (2) Gloucestershire               | 2.9                                     | 3.4                                     | 96                                                |  |
| (3) Herefordshire                 | 2.7                                     | 2.8                                     | 78                                                |  |
| (4) Kent                          | 2.9                                     | 3.5                                     | 93                                                |  |
| (5) Sussex                        | 2.8                                     | 3.6                                     | 96                                                |  |
| Mean                              | 2.78                                    | 3.37                                    | 91                                                |  |
| <b>Non-outbreak</b>               |                                         |                                         |                                                   |  |
| (6) Cambridgeshire                | 2.4                                     | 2.2                                     | 50                                                |  |
| (7) Devon                         | 2.3                                     | 1.6                                     | 26                                                |  |
| (8) Hampshire*                    | 2.4                                     | 1.4                                     | 41                                                |  |
| (9) Denbighshire                  | 2.1                                     | 1.7                                     | 7                                                 |  |
| (10) Oxfordshire                  | 2.2                                     | 2.1                                     | 23                                                |  |
| Mean                              | 2.28                                    | 1.80                                    | 29                                                |  |

\* Isolated from part of the county where the disease was not severe.

At Alice Holt 4-yr-old *Ulmus glabra* (seven replicates) and 3-yr-old *U. americana* (eight replicates) were inoculated with the ten isolates in randomized blocks. Spore suspensions were standardized with a nephelometer, *U. glabra* receiving  $7 \times 10^5$  spores and *U. americana*  $3.5 \times 10^5$  spores per tree. The trees were assessed by two independent observers after 6 weeks, and the extent of shoot dieback in *U. glabra* was measured after 8 weeks (Table 1). With both elm species the outbreak isolates were found to cause significantly more damage ( $P=0.001$ ) than non-outbreak isolates although the difference was more marked on *U. glabra*.

At Wageningen the elm material consisted of the highly susceptible clone P275 (*U. hollandica* Belgica), the moderately

resistant clone 274 (*U. hollandica* Commelin), and the highly resistant clone 496<sup>4</sup>. Techniques of inoculation were those standard in the elm breeding programme. The two English isolates were compared with isolates recently collected in Holland, two from Belgica and two from Commelin. The results in terms of disease index and leaf fall after 10 weeks are shown in Table 2.

**Table 2** Effect of English and Dutch Isolates of *C. ulmi* on a Range of Elm Clones 10 Weeks after Inoculation

| Origin <i>C. ulmi</i> isolates | Disease index Elm clones |                  |           | Percentage leaf fall Elm clones |                  |           |
|--------------------------------|--------------------------|------------------|-----------|---------------------------------|------------------|-----------|
|                                | Belgica (n=7)*           | Com-melin (n=10) | 496 (n=8) | Belgica (n=7)                   | Com-melin (n=10) | 496 (n=8) |
| <b>England</b>                 |                          |                  |           |                                 |                  |           |
| (1) Essex                      | 4.0                      | 3.7              | 3.2       | 100                             | 94               | 62        |
| (2) Gloucestershire            | 3.9                      | 3.8              | 2.6       | 100                             | 94               | 59        |
| <b>Netherlands</b>             |                          |                  |           |                                 |                  |           |
| <b>(a) from Belgica</b>        |                          |                  |           |                                 |                  |           |
| (1)                            | 1.8                      | 1.7              | 0.2       | 30                              | 20               | 3         |
| (2)                            | 2.1                      | 1.7              | 0.2       | 40                              | 16               | 0         |
| <b>(b) from Commelin</b>       |                          |                  |           |                                 |                  |           |
| (1)                            | 2.3                      | 1.8              | 0.8       | 47                              | 23               | 0         |
| (2)                            | 2.0                      | 1.7              | 0.2       | 40                              | 17               | 3         |

\* n = No. of trees.

The difference between the English outbreak isolates and the others was evident on all three clones, including 496, a point of particular significance as never before on this clone have symptoms exceeded those recorded in this experiment with the Dutch isolates. The poor performance of Commelin is also of importance. This tree has been planted widely in recent years, and its resistance to non-outbreak English isolates has been shown by repeated inoculation at Alice Holt<sup>5</sup>.

The implications of these results are considerable. It appears that an unusually aggressive strain of *C. ulmi* exists in Britain at present and that this is responsible, at least in part, for the current severity of the disease. It also appears that some of the clones bred in Holland for resistance to the disease are susceptible to this strain.

Work is in progress to investigate the aggressive isolates further and to study their performance against a full range of elm material.

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# BOOK REVIEWS

## *Octopus Neuroanatomy*

*The Anatomy of the Nervous System of Octopus vulgaris.* By J. Z. Young. Pp. xxxi+690. (Clarendon: Oxford; Oxford University: London, December 1971.) £11.

THE aim of neurobiology is to correlate function with structure. Without this correlation, attempts to understand central nervous function degenerate into theoretical exercises, as several important schools of psychology have found out. For understanding implies more than the capacity to manipulate; ultimately, one needs to know how the anatomy forces the system to behave as it does.

But biologists with the training and inclination to cope with both animal construction and animal behaviour are rare; it is far easier, and less vulnerable, to be a specialist, concentrate on one or the other and leave it to braver men to attempt the synthesis. Professor Young is one of the rare brave people; his previous books, from *A Model of the Brain* to *An Introduction to the Study of Man*, have already shown us that. One would expect *The Anatomy of the Nervous System of Octopus vulgaris* to be considerably more than an atlas, and, of course, it is. It is the most complete description of the anatomy of an invertebrate nervous system to be published to date. It represents one outcome of some thirty years of research, most of it by Young himself, and much of it hitherto unpublished. As originally conceived, by himself and Boycott (who writes the prelude to the present volume) in the early 1950s, it was intended to be a treatise on the brain and behaviour of cephalopods. It soon became apparent that new material was accumulating faster than it could reasonably be incorporated and that the embryo book was likely to grow too large for the normal processes of birth. So the scope of the treatise was successively reduced, first to an account of *Octopus* alone, and then to the present mainly anatomical work. The book still retains signs of its early development; the question of function is always present. Young never describes the anatomy of a part without also indicating any experimental evidence of its function, and where this is lacking he is prepared to make an informed guess. There is, naturally, a danger in this. With a structure as complex as a cephalopod brain, an enthusiast is liable to see what he expects to see, so that ideas of function are liable to give rise to facts about

anatomy. So far as one can tell, Professor Young never actually falls into the trap. He has seen it too often before, and he is too careful and honest an anatomist. So the description of what is there will last, even if some of the functional interpretations have to be modified, a comforting thought to anybody about to invest in what is, even by modern textbook standards, rather an expensive book.

There are probably not more than a dozen or so people in the world who will want this book, here and now, for their research purposes. For us, it is invaluable. We have needed it as a central reference point for the past twenty years and, indeed, have been referring to it ("in preparation" or "in press" on the basis of samples seen) for almost as long. But one can hope that a great many other people will soon find out they need it as well. Once the hard slog of basic anatomy has been done, all sorts of chances open up in the fields of electrophysiology, pharmacology and further electron microscopy. This book is clearly written in the hope that it will stimulate other people by pointing out the peculiar advantages of the central nervous system of *Octopus*. There is an opening section on gross anatomy which shows some of the more obvious of these. The brain is divided into some thirty discrete lobes, most of them sharply delineated one from another by a ring of cell bodies surrounding a central neuropil; the tracts connecting one part and another are generally clear, and there is a degree of completeness in the division between sensory sorting and motor control regions that might seem too good to be true to a neurobiologist accustomed to struggle with mammals. This animal even has separate sensory and learning systems for touch and vision!

Young deals with the lobes in functional groups. The level of description varies a little; there is, for example, considerable electron microscopic detail of the cellular arrangements in the vertical lobe (which has interested Young, because of its role in visual learning) and not a great deal at this level of analysis elsewhere. The parts concerned in sensory analysis and learning in general get a more thorough treatment than the higher motor control areas, where fewer behavioural and brain lesion experiments have been carried out to back up and stimulate the anatomical work. In addition to the straight "nervous system" account, there are chapters on established and

supposed neuroendocrine structures in and around the central nervous system, on the glia and blood vessels of the brain, on the eyes and the statocysts. All this is lavishly illustrated with photographs and line drawings; even the casual sightseer should have no trouble in finding his way about.

Finally, one should perhaps particularly recommend this book to anybody who still believes that the nervous systems of the more sophisticated invertebrates are somehow simpler than those of the higher vertebrates. Cephalopod brains, like those of insects, are not simpler than those of mammals, but different. Because of this, they are worth looking at, not as more elementary answers to the same problems, but because their solutions may be more susceptible to investigation for structural or physiological reasons. We know that mammals are difficult. Cephalopods are an alternative route to an understanding of some central nervous functions, and Young's massive book will make it possible for people to think about this.

MARTIN WELLS

## Semiconductors

*Defect Electronics in Semiconductors.* By Herbert F. Matare. Pp. xi+639. (Wiley: New York and London, August 1971.) £11.75.

It is always difficult to assess the appropriate point in time at which to write a book reporting the findings of research in a given field of work. Dr Matare is to be congratulated on having chosen now as the time to pull together the work of nearly two decades of research into the electronic effects of defects in semiconductors, in the form of a book. This is a field of increasing importance, especially in connexion with the current interest in thin semiconductor films in both polycrystalline and amorphous forms. It is therefore very welcome to find a book that gives a very thorough coverage, in the first seven chapters, of the types of defect including radiation-induced defects, their principal properties and the ways of describing these mathematically. An introductory chapter on the fundamentals of charge carrier transport includes the way in which the familiar theory is modified to deal with crystal defects, and this provides the basis of analysis of the electrical properties of defects started in chapter 8. The electrical effects of dislocations and grain boundaries are treated in depth in chapters 8, 9, 10 and 11, and these



are the core of the book. The treatment is basic, thorough and valuable although the examples chosen for illustrative solution seem a little limited. For example, field-effect modulation of conduction along a grain boundary is treated, but the case of modulation of conduction across such a boundary is not, and yet this is the basis of operation of the thin film transistor.

A chapter is devoted to the controlled growth of dislocations and provides much useful information for experimenters in this field. There is also reference to homo and hetero-epitaxial growth in which the important matter of misfit dislocations is discussed, although recent work on this, notably by Matthews, is not included. A final chapter is devoted to a rather brief treatment of amorphous materials which, alas, already looks rather dated.

A most useful appendix describes a wide range of methods of measurement of the electronic properties of semiconductor crystals and films.

In accordance with the current publishers' myth, problems are included at the end of each chapter, without answers being given. The number of misprints is regrettably high.

Altogether this is an invaluable book and is almost essential to the researcher working on semiconductor films.

J. C. ANDERSON

## NMR Spectroscopy

*Pulse and Fourier Transform NMR: Introduction to Theory and Methods.* By Thomas C. Farrar and Edwin D. Becker. Pp. xiv+115. (Academic: New York and London, February 1971.) \$7.50; £3.50.

*Analysis of High Resolution NMR Spectra.* By R. J. Abraham. Pp. x+324. (Elsevier: Amsterdam, London and New York, September 1971.) £11; Dfl. 95; \$26.25.

THE timely appearance of the book by Farrar and Becker should be welcomed by chemists working with high resolution NMR spectroscopy. At present the majority of them will be fully conversant with continuous wave NMR techniques but perhaps only vaguely aware of the potentialities of the pulse methods. This book sets out to redress the balance, and I think it should succeed. Mathematical rigour has been replaced by classical pictures in many instances, which should enhance the book's appeal to the less theoretically inclined chemist.

In continuous wave NMR experiments, nuclear relaxation is usually thought to be unimportant, except in the case of quadrupolar nuclei. In pulse experiments, however, relaxation is very important for all nuclei and this is duly

emphasized in the book. The opening chapters briefly cover the fundamentals of NMR and the pulse methods. These are followed by an interesting chapter dealing with the basic instrumental requirements for a pulsed NMR spectrometer. The importance of Fourier transform NMR is reflected in the fact that it has a chapter to itself. Emphasis is placed on the greater efficiency of pulse techniques for obtaining the same spectroscopic information that is available from continuous wave experiments, with the bonus of relaxation time measurements.

The final two chapters deal with methods of applying pulse techniques over a wide range of experimental conditions together with some examples of their application. These include binding of small molecules to polymers, molecular diffusion, chemical exchange and conformational changes in macromolecules.

In writing this book the authors set as a goal "a 100 page volume addressed to chemists and others familiar with high resolution NMR but with no background in pulse techniques". This they have achieved. Readers familiar with Dr Becker's previous book on high resolution NMR will not be surprised to find that this one is set out logically and written lucidly. It is perhaps unfortunate that the authors did not allow themselves more space to deal with further applications of pulse techniques and that they did not include more references from the rapidly growing literature on the subject. On balance, however, both authors and publisher are to be congratulated on producing a well written book at the right time.

*Analysis of High Resolution NMR Spectra* by R. J. Abraham faces competition from a number of texts dealing with this topic. As the author recognizes, the quantum mechanical basis of the analysis of high resolution NMR spectra was developed many years ago. Since then the emphasis has been on the development of more powerful analytical techniques. These are very adequately dealt with, together with some experimental aids to spectral analysis in the latter sixty per cent of this book.

Following a brief introduction to the basic concepts of NMR and the quantum mechanics necessary to discuss spectral analysis, two chapters are devoted to the analysis of some simple two, three, and four-spin systems. This part of the book has little new to offer the reader already familiar with the established textbooks in this area. The chapters on computer analysis of NMR spectra and sub-spectral analysis give the book a more authoritative nature. These chapters seem to be addressed to the experimental physical-organic chemist and as such they are to

be recommended. The complementary nature of sub-spectral and computer analyses is duly emphasized in these chapters.

Since the majority of users of computer programs in the analysis of high resolution NMR spectra are not the authors of these programs, a description of the operation of the program from the viewpoint of the user is important. With this in mind the author has given a comprehensive survey of the computer programs most widely available and some of their applications.

Those research areas in which Dr Abraham has made personal contributions are the ones most adequately covered in his book, the utility of which is enhanced by the examples at the end of each chapter. This book is to be recommended to those senior undergraduates and postgraduates concerned with the analysis of high resolution NMR spectra.

G. A. WEBB

## Neuropsychology

*Languages of the Brain: Experimental Paradoxes and Principles in Neuropsychology.* By Karl H. Pribram. Pp. xiv+432. (Prentice Hall: Englewood Cliffs, New Jersey, October 1971.) \$9.50.

THE author of this monumental work is professor of neuropsychology at Stanford University, California. The volume justifies its plural title and the sub-title is a concise summary of the inclusive contents. In his preface the author opens with a reference to "... paradoxes and puzzles which emerged unexpectedly from experimental results". This is the keynote, for much material, discoveries and hypotheses about brain function have often emerged from observations that seemed at first to be anomalous—or technical artefacts. Here, too, the author acknowledges the help and advice of more than a score of collaborators and text readers and the width and depth of his sources are carried through all the 432 pages.

In Part I, "A Two-process Mechanism of Brain Function", he introduces the notion that the neurone is only one unit, the other being the "neural junction" where slow potentials contribute independently of local receptive fields. From this arises the suggestion that the vast plasticity and storage capacity of the brain result from the modifications in the junctional mechanisms; mature neurones cannot divide and reproduce. The next step is analysis of the decremental and inhibitory processes. The fact of "habituation"—diminishing responses to monotonous signals—is the keystone of perception, for the converse responses to novelty and "orientation" are the essence of behaviour.

Details of the "coding" in neural patterns are discussed, with quotations from

many sources about the organization and modification of brain patterns according to experience. Finally, Pribram introduces the concept of "feedback" and "feed forward" in neural mechanisms and suggests the phrase: "Test-Operate-Test-Exit (TOTE)" to describe an elementary neural servomechanism. Here, the "Test" phase is related to the junctional, the "Operate" to the nerve-impulse component of brain function.

Part 2 is entitled "The Organization of Psychological Processes" and surveys widely and deeply every aspect of experience. There are more facts and original hypotheses than can be fairly summarized; one satisfying outcome is the re-establishment of the validity of subjective experience as an inference from the two-process concept. The bases of memory and the concept of feature-detection as a function of phase-coherence of junctional patterns are considered in detail, with the optical hologram as the distinct analogy. In a careful discussion of "feelings", Pribram suggests that some of the paradoxical results of experiments involving core-brain systems may be understood by postulating a "go-motivational" with a "stop-emotional" mechanism. He concludes with a cybernetic theory of motivation and emotion in which cortical influence on deeper structures is invoked from empirical factors.

These considerations are developed in Part 3: "The Neural Control and Modification of Behavior". This deals with movements and actions and the basic concepts of contingency, expectancy and innate competence. Here the author builds a link between fields as remote as ethical categories and the intimate details of neurochemistry, with reference to learning, achievement and the means-end relation in reinforcement.

Part 4 is perhaps the most challenging; it extends the discussion to analysis and learned conjectures on animal and human communication. This is too intricate and wide-ranging to summarize intelligibly; the great words talk and thought are discussed with scholarly respect, with reference to the accepted facts of anatomy and physiology, and it ends with a gentle and penetrating essay on the regulation of human affairs. Here, Pribram expresses a "post-critical, biologist's view", which he describes as "a form of constructional realism" with a proposal for a "structural pragmatism".

As well as the great mass of experimental material, discoveries and penetrating conjectures, there is a personal epilogue that opens with the sentence, "The making of this book held many surprises for me". This will be true, in the happiest sense, of the reader, however experienced in brain research.

There are twenty pages of bibliography and excellent author and subject indexes.

W. GREY WALTER

## Supplying Voltage

*Power Supplies for Electronic Equipment.* By J. R. Nowicki. Vol. 1: Pp. xxi+321; £6.50. Vol. 2: Pp. xxi+224; £5.80. (Leonard Hill: London, September 1971.)

IN these two volumes Mr Nowicki has set out to provide a comprehensive treatment of the subject; only rotating machines and other mechanical devices seem to be omitted. The first volume starts with a detailed discussion of the characteristics of semiconductor rectifiers and transistors; particular attention is given to the thermal and other limitations which affect their use in power supplies. He then deals with methods of rectifying alternating voltages to produce d.c. supplies and includes a treatment of polyphase rectifiers used for high power applications. Inversion from d.c. to a.c. and the conversion from one direct voltage to another are also dealt with. Almost every method which has been used is considered, including "fly-back" and r.f. oscillator methods of providing high voltage supplies for television receivers. Every method described is analysed in some detail.

Volume two deals mainly with regulated power supplies and a bewildering variety of circuits is given together with an analysis of their operation. Here again nothing seems to have escaped his notice and he deals at some length with the problems of obtaining a stable reference voltage and with the methods of protecting circuits against overloads. Apart from the many variations of the simple shunt and series stabilizing circuits, he devotes a chapter to circuits using switching methods to achieve close voltage control; this chapter is, however, rather more descriptive in nature than most of the others. The last two chapters are devoted to a more detailed description of inverter circuits. The two volumes contain a great deal of useful material. It is likely to be of more value to an engineer who already has some experience with power supplies than to a beginner. This is because the author gives a great deal of impartial description and analysis. Though he compares one circuit with another he leaves it to the reader to form his own preferences. In a topic such as this it would be helpful if the author from his own expertise would provide a "child's guide" to the choice of the simplest circuit which would give a desired performance.

On the whole the two volumes provide a very useful reference work.

W. E. LOVERING

## Laser Technology

*Laser Applications.* Vol. 1. Edited by Monte Ross. (Academic: London and New York, September 1971.) \$16; £7.45.

FIVE experts here describe some of the developments in their own fields in the last ten years. The result is a very useful and readable book which helps to fill in the gap between laser research and applications in useful products and instruments. The volume is well produced, with adequate American references. Misprints are few.

Each author treats his section from his own standpoint, giving his first-hand interests most space, though in some cases the account seems to support an implication that the author moved to other work some time ago. Thus the scope of each section is not exhaustive, and plenty of room remains for sections in future volumes giving complementary and updated contributions within the compass of the titles of the present sections.

The volume is in five sections. Brian J. Thompson gives a straightforward introduction to the fundamentals of holography, followed by a selection of developments. Particle size analysis is followed through to several well developed systems; it would be interesting to hear what these systems have achieved and how the techniques compare with competing technologies.

A clear and detailed account of some of the measurements made at the ESSA Research Laboratory is given by James C. Owens in his section on "laser applications in metrology and geodesy".

The daunting problems which arise, and the ingenious techniques evolved in the development of the laser gyro, are described by Frederick Aronowitz. The discussion seems to be limited to early models, perhaps for security reasons.

In the discussion of machining and welding, by Leland A. Weaver, too much space is devoted to a survey of lasers, and the result is a rather restricted account.

Monte Ross discusses the basis of choice of communications systems which suit existing requirements, but scarcely mentions terrestrial possibilities. His good account of laser communications is inevitably restricted because experience in the field of communication in space had hardly begun at the time of writing, so that little is said about real systems.

The volume title may be slightly misleading. "Applications", in the sense of proved products or techniques, are not described: this volume describes only laser technology aimed at potential applications. Perhaps future volumes will pursue the development of some of these topics into the final stage, giving



accounts which will set out advantages and disadvantages against relative costs, with evidence of commercial or other utilization. J. W. C. GATES

## Chemical Bonding

*The History of Valency.* By C. A. Russell. Pp. xii+373+12 plates. (Leicester University: Leicester, October 1971.) £5.50.

THIS volume, by focusing on the subject of chemical bonding, develops not only the history of valency but much of the history of modern chemistry itself. From the time that Davy and Berzelius were grappling with the problems of chemical composition up to the present day, the chemist has necessarily had to be concerned with the manner in which elements combine. Even before Dalton introduced the atomic theory there were various affinity concepts which sought to deal with the problem of combination. Following the development of a viable chemical atomism, the uniformity of atomic combination inevitably led to a concept of atomic saturation, or equivalency, or simply valency. Yet the development of the concept appears surprisingly slow when viewed in retrospect—but necessarily so when it is remembered that the atomic weight problem was only settled after 1860.

Russell has made an unusually fine study. Not only does he expertly develop the background coming out of Berzelius's dualistic theory, but shows the complementarity of the apparently antagonistic radical and type theories in creating the conceptual framework out of which valency ideas could develop. The treatment of details of the several variants of radical and type theories is unique in its clarity. Many writers on the problems of formulation during the eighteen thirties and forties have stumbled badly but the author has clearly mastered the problems of the period.

Also impressive is the way in which Russell reveals the dynamic nature of valency theory. One never gets the impression that the theory settled into dogma, but that critical problems in experimental chemistry always pushed the theoreticians to a re-examination of their position. Thus, even Kekulé's desire to establish a single valency for each element never froze valency into stasis. The problems of unsaturation, aromaticity, geometric isomerism, and tautomerization all came to be handled within the concept of the tetravalent carbon atom, but the structural problems associated with inorganic compounds could not be handled as easily. In fact, the early attempts to depict inorganic structures in terms of invariable

valencies, using organic structures as models, were a fiasco. It was only after variable valencies were accepted, along with the concept of secondary valencies, that Alfred Werner was able to formulate complex ions in a satisfactory manner.

The chapter on aromatic compounds and the origin of the hexagon formula for benzene is particularly good. The author has a clear treatment of the failure of the Ladenburg prism formula to explain certain experimental facts and has a full treatment of the various hexagonal alternatives to Kekulé's formula.

The last part of the book treats the subject of electrochemistry and the role of the electron in chemical bonding. The static models of Lewis and Langmuir introduced a respect for the stable octet and the electron pair bond which would create new problems in bonding theory. The role of the new quantum mechanics in the development of valence bond theory and the alternative molecular orbital theory brings the history to a close which, one suspects, does not represent the final word on the subject.

AARON J. IHDE

## Marine Life

*Fertility of the Sea.* Edited by John D. Costlow. Vol. 1: Pp. xi+308. Vol. 2: Pp. xi+309-622. (Gordon and Breach: New York, London and Paris, December 1971.) £14.60 the set of two volumes.

THESE two volumes contain thirty-eight papers given during a symposium in São Paulo, Brazil, in December 1969. They include general reviews, such as "Alternative sources of food in the sea" by J. E. G. Rayment, "Oceanography and its relation to marine organic production" by E. C. and K. G. LaFond and "River-ocean interactions" by E. D. Goldberg who argues powerfully for increased biological and inorganic chemical studies of estuaries and neighbouring sea waters. Narrower research reports include titles such as "Seasonal variations in the physico-chemical composition of seawater in Paradise Harbour—West Antarctic" and "Echinoderms of the Ilha Grande region". There is an emphasis on South American waters but there are, also, papers dealing with the Indian Ocean, the North Atlantic and a "unique coastal environment off Oregon, USA".

Most of the papers are biological but there is an essay on the mixing of water masses and another on studies with drift bottles off the Brazilian coast. A few papers deal with experiments, including an interesting account by T. J. Smayda of the use of a diatom, in

assays of natural waters from three standard depths at four stations widely spaced between 20° and 40° N in the Atlantic. At a depth of 100 m at all stations, he found what he calls "bad" water in which the diatoms died but growth was stimulated by the addition of inorganic nutrients and vitamins, whereas it was inhibited by adding nutrients to water from the surface or 50 m. In a most useful paper on nitrogen fixation, W. D. P. Stewart describes experimental methods and reviews what is known about algae, heterotrophic and photosynthetic bacteria and yeasts. He discusses the factors governing nitrogenase activity in the sea, as well as the production of extracellular products, and puts the whole topic into an ecological and physiological perspective: a noble contribution when it is remembered that "it is only within the past decade that biological nitrogen fixation in the sea had been established with certainty".

As these diverse examples suggest, the theme of the symposium, "The Fertility of the Sea", was interpreted very broadly, and this, coupled with the large number of papers, compels a superficial treatment of many topics; for example, "Marine pollution and its biological consequences" is dealt with in eight pages.

There are two kinds of symposia. In the first, a limited number of acknowledged experts is invited to speak on firmly defined subjects, selected to complement each other and, collectively, forming a synthesis of a single field of research. The published record of this kind of symposium is often extremely valuable and may, indeed, constitute the definitive text to which research workers everywhere will turn; thus the symposium is as valuable for those who did not attend it as for those who did. The second kind of symposium is a meeting of people (often too many of them), each reading his favourite paper or filling in with an account of his current project; the papers tend to be a mixture of good and bad, generalizations and narrow specialisms. There is little that an editor can do to synthesize a theme out of the uncoordinated topics and even less that he can do to raise the uneven quality of the contributions.

This symposium falls into the second category; its value is not for the reader of these volumes but, presumably, for those who assembled in São Paulo—and the foreword suggests that it was highly successful in this respect, as well as being a tribute to the advancement of marine science in South America. It is arguable, however, that it would have been wise to have left it at that without committing the contributions to formal (and fairly expensive) publication.

R. S. GLOVER

# CORRESPONDENCE

## Whale Conservation

SIR,—You draw welcome attention to the fact that, notwithstanding recent concern about the effects of pollutants like DDT and others on various species of animals, the direct effects of too intensive killing have been the major cause of the observed declines in species of birds, whales or fish, and that more emphasis should be given to the studies of those direct effects (*Nature*, 233, 79; 1971). However, in doing so, you appear not to be aware of much of the work and progress that has already been achieved. As scientists who have been associated with the work of various international commissions both before and since joining FAO, we should like to make some personal observations.

We would in particular like to comment on the statements regarding Antarctic whales (see also *Nature*, 232, 80; 1971). It is certainly not true that at least two species of whale have been made extinct in the Antarctic, or indeed anywhere in the world, since the Atlantic grey whale was exterminated many centuries ago. The blue whale might have been exterminated if the slaughter of the early postwar period had continued, but since being protected in 1965 it has almost certainly increased, and now numbers several thousands—not between 100 and 1,000 as stated—and is regularly seen by whalers and research ships in the Antarctic and elsewhere.

While the International Whaling Commission has become a favourite and easy target of attacks, these criticisms do not do full justice to the commission's record in the last six years. While around 1965 the IWC was failing badly to bring the catches down into line with the figures recommended by the scientists, the catches were some three or more times the sustainable yield, and various stocks had declined seriously, the quotas now set by the IWC have been cut to a fraction of these former levels, and according to some scientists are below the sustainable yield of the stocks. The result is that, apart from the species which are fully protected, and perhaps with the exception of some local stocks of other species, the fin whale is the only species of which the present stock size is far below the level giving the maximum sustainable yield. The present Antarctic quotas are above some estimates of the current sustainable yield, though the excess is small, and if these estimates are correct the quotas would lead to only a slow further decline on the stocks.

Admittedly, much remains to be done,

and in particular the member countries of the IWC are still failing to implement the international observer scheme, and little or nothing is being done to rebuild the Antarctic fin whale stocks to a level where they can provide a high sustained yield—maintaining them at around the present level is not good enough. Furthermore, problems have arisen from whaling carried out by countries not members of the commission. For example, the 66 blue whales referred to as being killed in 1967, after full protection was given to them, were killed by vessels of countries not belonging to the IWC and over which the member states of IWC had no authority, and certainly some action needs to be taken to cope with this kind of problem.

As regards fish in the North Atlantic, considerable efforts have been made within the framework of the International Commission for the North West Atlantic Fisheries and the North East Atlantic Fisheries Commission to set up international controls of the intensity of fishing in the North Atlantic, including a special meeting of NEAFC at ministerial level in Moscow last December. Progress is indeed slow, but, contrary to the impression given in your journal, limits have been set by ICNAF to the intensity of fishing on haddock in several parts of the North West Atlantic, including the Scotia Bank.

Like the IWC, these and other international bodies concerned with management and conservation of living marine resources need better support so that they can improve their performance, and particularly the speed with which they can react to the rapid changes in modern fisheries. Such support should include better facilities for the collection and analysis of information regarding the stocks, and greater authority to ensure compliance with necessary conservation action.

Yours faithfully,

L. K. BOEREMA  
J. A. GULLAND

Department of Fisheries,  
FAO, Rome

## Geological Abstracts

SIR,—Joel Lloyd (*Nature*, 235, 347; 1972) has tidied up some errors of fact in your original comment on this matter, and then gone on to add some unsubstantiated comments of his own. Since the division of opinion between us is deep and is relevant to the current

urge to mechanize bibliographic services in other fields, I hope you will allow me to reply.

I do not quarrel with the suggestion that the American Geological Institute is providing a fairly comprehensive service, and that in the context of high machine costs it is wise to restrict itself to citations and not fill its magnetic tapes with abstracts. I agree that the abandoned USGS services were cheaper than they need have been, but as a run-on print from a bibliographic service paid for and justified by the internal needs of the USGS, their marginal cost was never very high. In any case, the price for his own service is also "unrealistic" and incapable of covering its costs at the present price-tag or any other, higher or lower. As in many subjects, the value placed on information does not cover the costs of the complex systems that information specialists dream up these days. Why is it that the consumers are at fault in being reluctant to pay—why cannot the information industry realize that a less sophisticated system that can cover its costs is an alternative worth considering seriously? Comparison with the costs of other more advanced and information-conscious subject fields is irrelevant; services for geologists are only going to be paid for by geologists.

To Dr Lloyd the fact that a service is not comprehensive (that is, subject-wide) and not machine-based destroys all its claims to be a useful service. The fact is that Dr Howie and I can outsell Dr Lloyd just because we each produce (very different) services that people find useful and that people do afford to buy. In time such services may grow and provide a more comprehensive service, and they will gradually become more mechanized as the users find they need more sophisticated services. Last year *Geo. Abstracts* spent an appreciable sum of money translating and publishing abstracts of the Russian literature, normally very inadequately covered by us. To information specialists this was an obvious and overdue extension of our service. The experiment produced two letters in favour (one of these from another bibliographer hardly counts); the remainder were strongly against. The consumer view was clear; why clutter up the *Abstracts* with this material we don't want? Professionally I deplore the attitude, but are we right to burden our consumers with expensive services they do not want? It seems to me that this is a central question as expensive and heavily subsidized services bemoan the lack of consumer



demand. Is this entirely the consumer's fault?

Yours faithfully,

K. M. CLAYTON

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## Forensic Science

SIR,—Dr Curry in his article on "Recent Developments in Forensic Science" (*Nature*, 235, 369; 1971) errs on the side of extreme conservatism when he

asserts that the existing techniques of blood grouping and enzyme measurement can occasionally give a discriminatory power of 1 in 1,000. This laboratory uses routinely in case-work three serological systems (ABO; MN; Rh) and up to six enzyme systems (AK; ADA; PGM; 6PGD; G6PD; PCE) plus haptoglobin and haemoglobin. Discrimination of 1 in 1,000 is commonly obtained but from time to time values as high as 1 in  $10^6$  have arisen.

Dr Curry has apparently overlooked the advantages of atomic absorption spectrometry which are being exploited in several forensic laboratories. Considerable developments are also being

made in scanning electron microscopy particularly in conjunction with electron probe X-ray microanalysis. Such equipment has been in use in the MP Laboratory for nearly a year and has been very useful in the elemental analysis of fragments of paint, glass and metals even when the cross section of the sample analysed is as small as 10  $\mu\text{m}$ .

Yours faithfully,

R. L. WILLIAMS

*Director*

*The Metropolitan Police  
Forensic Science Laboratory,  
2 Richbell Place, London WC1*

# Obituary

## Sir Frederick C. Bawden



SIR FREDERICK CHARLES BAWDEN, FRS, Director of Rothamsted Experimental Station and Treasurer of the Royal Society, died on February 8 at the age of 63.

Frederick Bawden was a scholar of Emmanuel College, Cambridge, and he graduated with first-class honours in botany. Rather than enrol for a PhD—a degree he never did acquire—Bawden instead obtained a diploma in plant pathology at the Cambridge School of Agriculture. His first appointment in 1930 was as research assistant to the formidable Redcliffe N. Salaman. Salaman, the son of a wealthy Jewish family and a medical man, was originally engaged in medical research at the London Hospital, but he contracted tuberculosis at the age of 30 and was advised to lead an outdoor existence. Living quite near Cambridge at Barley, Salaman became interested in the scien-

tific genetics and breeding of the potato, on which he became a leading authority, and subsequently he developed an interest in the virus diseases of potatoes. As a consequence he persuaded the Ministry of Agriculture in 1927 to establish and finance the Potato Virus Research Station (later to become the ARC Virus Research Unit), which was situated on the edge of Cambridge University Farm, and at the time consisted only of a few glasshouses and a potato shed. There Bawden was to meet Salaman's other colleagues, who included Kenneth Smith, the station's entomologist, and to receive a thorough grounding in virus diagnosis and transmission by grafting, sap inoculation and insect transfer. The work of a research assistant consisted very largely of routine work of this type and of recording the results, often photographically, under the most primitive conditions. The darkroom was built onto the potato shed and was hot in summer and freezing in winter. There Bawden was to make his only contribution to scientific photography. Taking advantage of the infrared reflexion band of chlorophyll, he managed to get good photographs of necrotic virus symptoms on plant leaves using infrared plates<sup>1</sup>.

Subsequently he became interested in the diagnosis of virus diseases using the immunological methods that had just been developed. This resulted in a fruitful collaboration with E. T. C. Spooner of Cambridge University Pathology Laboratory. Intrigued as to the nature of the viral antigens present in diseased plants, Bawden also became a close collaborator with a young biochemist, N. W. Pirie, who was Demonstrator in Sir Frederick Gowland Hopkins's famous biochemistry laboratory. Bawden and Pirie, who complemented each other in their knowledge and

abilities, soon found that the antigen of Kenneth Smith's potato "X" virus had many of the properties characteristic of proteins<sup>2</sup>.

At about this time W. M. Stanley at the Rockefeller Institute, Princeton, published his famous paper on the nature of the tobacco mosaic virus, making the claim that the virus was a crystalline globulin<sup>3</sup>. Repeating this work Bawden and Pirie, in collaboration with J. D. Bernal and I. Fankuchen, showed that the tobacco mosaic virus was a long thin nucleoprotein rod at least 1000 Å long, having a regular structural repeat along its length every  $3 \times 22$  Å and containing about 5 per cent of ribonucleic acid<sup>4</sup>. The "crystals" of Stanley were found to be liquid crystals. This one paper more or less established modern biochemical virology as well as forming one of the foundations of molecular biology. It was followed by a monumental paper in the *Proceedings of the Royal Society*, which contained so much new information that even now it is well worth consulting<sup>5</sup>. In particular they noted at this time that the nucleic acid of the virus was a much larger molecule than was expected on the basis of the chemical evidence available.

The presence of nucleic acid in the virus was a matter of some controversy for some time, but Bawden and Pirie went on to purify potato "X" virus, cucumber viruses 3 and 4 and Kenneth Smith's tomato bushy stunt virus, all of which were found to contain ribonucleic acid. The cucumber viruses proved unexpectedly to resemble tobacco mosaic virus both physically and chemically though they had no known host in common with the latter. This was the first instance in which virus relationships were finally recognized on the basis of chemical and

physical properties, a procedure which has wide application nowadays. Tomato busby stunt proved to be very different from the others, forming beautiful rhomboic dodecahedral crystals, and having "spherical" particles which had a considerably higher proportion of ribonucleic acid. This virus has been the subject of much further study, particularly by X-ray crystallography. Subsequently several unstable viruses were also purified.

At this time much of the interest in these new discoveries centred around the fact that viruses were evidently quite simple in their structure and, though obviously able to multiply and so cause disease, were also capable of forming crystalline arrays of one kind or another. This raised philosophical questions as to the nature of life itself, questions that were only resolved after the recognition of the biological functions of the nucleic acids.

In 1936 Bawden left Cambridge to become Virus Physiologist at Rothamsted Experimental Station in Harpenden, a position subsequently held by Pirie and which caused them both some amusement, because at that time viruses had no evident physiology. The Director of Rothamsted was Sir John Russell, who took a great interest in Bawden's work and promoted him to Head of the Plant Pathology Department in 1940. Elected to the Royal Society in 1949, he shortly afterwards became Deputy Director and finally, in 1959, Director of Rothamsted, the oldest and most famous agricultural research establishment in the world—a position which he held until his death.

Although he was plunged into administration at a fairly early age, Bawden kept up with his own work on viruses, which he later said "was for long my livelihood and has recently been my hobby"<sup>6</sup>. His group at Rothamsted, which was very successful and productive, included among many others A. Kleczowski, B. Kassanis and Marion Watson. His list of original scientific publications is enormous. Apart from the work already mentioned he was active in the study of the infection process of viruses, the behaviour of vectors, the effects of radiation on viruses and many other aspects of virology. His first textbook, *Plant Viruses and Virus Diseases*, was published in 1939 and was so different in its approach to its subject matter that one reviewer felt obliged to remark, "This forms the theme of his book, which might with advantage have been entitled *Recent Research on the Nature of Plant Viruses*. Thus it differs, particularly in outlook but also in treatment, from the recent textbooks of virus diseases, which are primarily concerned with the economic host plants: this one especially considers the infective agents"<sup>7</sup>.

Always an entertaining speaker, he spoke with a soft accent betraying his native county of which he was justly proud. At an international meeting at which speakers had been praising the quality of their countries' agricultural products, after commenting on the quality of British seed potatoes, he disclaimed any association with Scotland and Ireland and added as a characteristic aside, "I come from Devon, where they grow the best cider in the world, the best cream in the world and the best men in the world." So popular were his lectures that on the occasion of his Loewenhoek Lecture at Burlington House in 1959, not only was all sitting and standing room filled but many would-be listeners failed altogether to get admission to the Royal Society's lecture room.

He was President of the Association of Applied Biologists in 1965 and of the Society for General Microbiology from 1959–61, of the Institute of Biology, the British Insecticide and Fungicide Council (1966–68) and the British Crop Protection Council. At the First International Plant Pathology Congress in London, of which he was also President, he was given the Elvin C. Stockman Award of the University of Minnesota. He was the recipient of many other awards and honorary degrees, which were given not only because of his scientific distinction, but also for the vast amount of work which he carried out all over the world investigating serious local plant disease problems. He was also a member of the Natural Environment Research Council.

He was knighted in 1967 and became a Vice-President and Treasurer of the Royal Society in 1968.

The last few weeks of his life were devoted to a large extent in attempting to expose what he felt were the inadequacies of the arguments put forward by Lord Rothschild in his contribution to the Government Green Paper on Research and Development, a task which he undertook with characteristic forthrightness and humour<sup>8</sup>. He will be greatly missed by very many of us.

<sup>1</sup> *Nature*, **132**, 168 (1933).

<sup>2</sup> *Brit. J. Exp. Path.*, **17**, 64 (1936).

<sup>3</sup> *Science*, **81**, 644 (1935).

<sup>4</sup> *Nature*, **138**, 1051 (1936).

<sup>5</sup> *Proc. Roy. Soc., B*, **123**, 274 (1937).

<sup>6</sup> *Ann. Appl. Biol.*, **58**, 1 (1966).

<sup>7</sup> *Nature*, **145**, 122 (1940).

<sup>8</sup> *Nature*, **235**, 7 (1972).

chair of social and preventive medicine at Middlesex Hospital Medical School, University of London; Dr B. E. Ryman, Royal Free Hospital School of Medicine, has been appointed to the chair of biochemistry at Charing Cross Hospital Medical School, and Professor H. K. Weinbren, University of Nottingham, has been appointed to the chair of histopathology, Royal Postgraduate Medical School. The following titles have been conferred within the University of London: professor of anthropology of Africa and the Near East, on Dr A. Cohen in respect of his post at the School of Oriental and African Studies; professor of philosophy of science, on Dr H. R. Post in respect of his post at Chelsea College; professor of applied pharmacology, on Dr R. G. Spector in respect of his post at Guy's Hospital Medical School; professor of virology, on Dr H. Stern in respect of his post at St George's Hospital Medical School; professor of the history and applications of mathematics, on Dr G. J. Whitrow in respect of his post at Imperial College.

## International Meetings

March 28–29, **The Education of Human Ecologists**, Huddersfield (Dr P. F. Rogers, Biology Section, The Polytechnic, Queensgate, Huddersfield HD1 3DH).

April 5–7, **High Energy Collisions**, Oxford (Dr G. Manning, Rutherford Laboratory, Chilton, Didcot, Berkshire).

April 6, **Introduction to Statistics for all Chemists**, London (Assistant Secretary, Society of Chemical Industry, 14 Belgrave Square, London SW1).

April 11–13, **Inter-disciplinary Investigations of the Brain**, Oxford (Dr J. P. Nicholson, Physics Department, Westminster Hospital, Page Street Wing, London SW1).

April 13–14, **Biochemical Aspects of Industrial Pollution**, Cardiff (The Biochemical Society, 7 Warwick Court, Holborn, London WC1).

April 17–20, **Seed Ecology**, Nottingham (W. Heydecker, University of Nottingham, School of Agriculture, Sutton Bonington, Loughborough LE12 5RD, Leicestershire).

## Erratum

IN the title of the article by R. N. Thompson on page 106 of this issue, the word andesite should be in inverted commas. This correction should also be made in the first subtitle and on page 107, lines 1, 10, 14, 17 and 20 of column 1; page 108, line 4 of column 1 and lines 6 and 38 of column 2; page 109, lines 4, 18, 32 and 34 of column 1 and lines 7, 21 and 32 of column 2; page 110, lines 4, 13 and 15 of column 1.

## Announcements

### University News

Dr D. L. Miller, Central Public Health Laboratory, has been appointed to the



# Reports and Publications

not included in the Monthly Books Supplement

## Great Britain and Ireland

Proceedings of the Royal Irish Academy. Vol. 71, Section A, No. 7: Runge Domains in Banach Spaces. By Sean Dineen. Pp. 85-89. 10p. Vol. 71, Section B, No. 18: Growth and Food of Brown Trout *Salmo trutta* (L) in Irish Waters. By M. Kennedy and P. Fitzmaurice. Pp. 269-352+plates 8-12. £1.45. (Dublin: Royal Irish Academy, 1971.) [31]

Cotton Research Corporation 1921 to 1971. (In celebration of the Golden Jubilee of the Cotton Research Corporation, and of the donation of its Cotton Research Station, Namulonge, to the Government of Uganda on 1st January 1972.) Pp. 28. (London: Cotton Research Corporation, 1971.) [31]

Visual Media in Geography and Geology—a Conference Report, 30th January 1971, National Film Theatre, South Bank, London. Pp. 57. (London: British Universities Film Council, 1971.) 65p. [31]

British Medical Bulletin. Vol. 28, No. 1 (January 1972): Symposium on Research in Diseases of the Tropics. Pp. 1-102. (London: The British Council, 1972.) £2.25. [31]

Young Fabian Pamphlet No. 28: The Universities—Pressures and Prospects. By Colin Crouch and Stephen Mennell. Pp. 47. 45p. Fabian Research Series, No. 299: Communists: All Revisionists Now? By S. F. Kissin. Pp. 36. 40p. (London: Fabian Society, 1972.) [41]

Wool Industries Research Association. Report and Accounts 1971. Pp. 26. (Leeds: WIRA, 1972.) [41]

The Zoological Record, 1968, Vol. 105, Section 2: Protozoa, Part 1 (Recent) and Part 2 (Fossil). Compiled by the Staff of the Zoological Society of London. Pp. 186. £3.50. 1969, Vol. 106, Section 11: Trilobites. Compiled by Sir James Stubblefield, FRS. Pp. 55. £5. (London: The Zoological Society of London, 1971.) [51]

The Scientific Proceedings of the Royal Dublin Society. Series A, Vol. 4, No. 6: Cleavage Polyembryony in Conifers and Taxads—a Survey. 1. Podocarps, Taxads and Taxodioids. By J. Doyle and M. Brennan. Pp. 57-88. £1. Vol. 4, No. 7: Ordovician Ash-Fall Tuffs from Co. Waterford. By C. J. Stillman. Pp. 89-102+plates 4-6. 50p. Vol. 4, No. 8: A New Look at the Rossire Complex. By Michael D. Max and N. B. Dhonau. Pp. 103-120+plate 7. 50p. Series B, Vol. 3, No. 1: Age and Growth of Pike in Five Irish Limestone Lakes. By J. J. Bracken and W. S. T. Champ. Pp. 1-34. 75p. Vol. 3, No. 2: Limits of Weichsel Glacial Deposits in the South Tipperary Area. By T. F. Finch. Pp. 35-42. 36p. Vol. 3, No. 3: The Control of the Fox, *Vulpes vulpes* (L.), Populations in Northern Ireland. By J. S. Fairley. Pp. 43-48. 30p. Vol. 3, No. 4: Seasonal and Vertical Distribution of the Arthropod Fauna of an Old Grassland Soil. By J. P. Curry. Pp. 49-72. 75p. Vol. 3, No. 5: *Myzus persicae* (Sulz.) and other Aphids Occurring on Sugar Beet in Ireland and Their Importance as Vectors of Virus Yellow. By R. Dunne. Pp. 73-80. 40p. Vol. 3, No. 6: The Occurrence and Control of Pigmy Mangel Beetle, *Atomaria linearis* (Steph) in Ireland. By A. M. Feeney. Pp. 81-86. 40p. Vol. 3, No. 7: A Comparison of Soils on Coal Measures in S.W. Ireland and S.E. Wales. By C. B. Crampton and T. F. Finch. Pp. 87-100. 45p. (Dublin: The Royal Dublin Society, 1971.) [52]

Bulletin of the British Museum (Natural History). Entomology. Vol. 26, No. 8: A Revision of Francis Walker's Types of North American Empididae (Diptera). By Kenneth George Valentine Smith. Pp. 345-370+3 plates. £1.20. Zoology. Vol. 22, No. 2: The Clupeoid Fishes Described by Francis Day. By P. K. Talwar and P. J. P. Whitehead. Pp. 57-85+2 plates. £1.20. Vol. 22, No. 3: Fine Structure of *Bodo saltans* and *Bodo caudatus* (Zoomastigophora: Protozoa) and Their Affinities with the Trypanosomatidae. By B. E. Brooker. Pp. 87-102+6 plates. (London: British Museum (Natural History), 1971.) [61]

Fabian Research Series No. 298: Collective Bargaining and Inequality. By Jim Skinner. Pp. 20. (London: Fabian Society, 1971.) 20p. [71]

The Edinburgh School of Agriculture. Experimental Work 1970. Pp. 146. (Edinburgh: Edinburgh School of Agriculture, 1971.) 50p. [71]

Doom or Deliverance?—The Dogmas and Duties of a Technological Age. By the Right Reverend Hugh Montefiore. (The Rutherford Lectures.) Pp. 30. (Manchester: Manchester University Press, 1972.) 49p net. [101]

Friends of the Lake District. Report and News Letter, January 1972. Pp. 18. (Kendal: Friends of the Lake District, 27 Greenside, 1972.) [111]

The Medical Research Council of Ireland. Annual Report for the year ended December 31, 1970. Pp. 79. (Dublin: The Medical Research Council of Ireland, 1972.) 25p. [111]

Science Research Council. Radio and Space Research 1968-1970—The Report of the Director of the Radio and Space Research Station. Pp. iii+59. (London: HMSO, 1971.) 80p net. [121]

Progress Reports from Experiment Stations. Season 1970-71. Uganda. Pp. 82. (London: Cotton Research Corporation, 1971.) 30p. [121]

Beef Processing and Marketing: Proceedings of an International Symposium held in Dublin on April 28 and 29, 1971. Edited by D. E. Hood. (Irish Livestock and Meat Commission; Institute of Food Science and Technology.) Pp. 108. (Dublin: An Foras Taluntais, The Agricultural Institute, 1971.) £1.50. [121]

## Other Countries

Smithsonian Contributions to Zoology, No. 58: Keys to the Hawaiian Marine Gammaridea, 0-30 Meters. By J. Laurens Barnard. Pp. iii+135. (Washington, DC: Smithsonian Institution Press, 1971. For sale by US Government Printing Office.) \$1.75. [51]

US Department of the Interior: Geological Survey. Techniques of Water-Resources Investigations of the United States Geological Survey. Chapter A2: Determination of Minor Elements in Water by Emission Spectroscopy. By Paul R. Barnett and E. C. Mallory, Jr. Pp. v+31. (Washington, DC: Government Printing Office, 1971.) 50 cents. [61]

Australia: Commonwealth Scientific and Industrial Research Organization. Division of Soils Technical Papers. No. 8: Basic Exchangeable Cations in Soils. By B. M. Tucker. Pp. 22. No. 9: The Calculation of Results in Electron-Probe Microanalysis. By A. C. Oertel. Pp. 26. No. 10: A Storage and Retrieval System for Micromorphological Descriptions of Soils. By (The Late) J. M. Norris, S. W. Cumpston and J. R. Sleeman. Pp. 20. (Melbourne: CSIRO, 1971.) [61]

Australia: Commonwealth Scientific and Industrial Research Organization. Annual Report of the Division of Animal Health for 1970. Pp. 95. (Melbourne: CSIRO, 1971.) [61]

Smithsonian Contributions to Earth Sciences. No. 7: Geology of the Lincoln Area, Lewis and Clark Country, Montana. By William G. Melson. Pp. 29. 40 cents. Smithsonian Contributions to Paleobiology. No. 9: Catalog of Type Specimens of Invertebrate Fossils—Conodonts. Compiled by Frederick J. Collier. Pp. 256. \$2.50. Smithsonian

Contributions to Zoology, No. 84: Butterflies of the Genus *Vanessa* and of the Resurrected Genera *Basaris* and *Cynthia* (Lepidoptera: Nymphalidae). By William D. Field. Pp. 105. \$1.50. No. 107, A New Approach in the Analysis of Biogeographic Data. By James A. Peters. Pp. 28. 40 cents. No. 109: Partial Revision of the Genus *Silvestris* Kinberg (Polychaeta: Sigalionidae) with Diagnoses of Two New Genera. By Marian H. Pettibone. Pp. 40. 50 cents. (Washington, DC: Smithsonian Institution Press, 1971. For sale by US Government Printing Office.) [61]

Smithsonian Contributions to Zoology, No. 104: Revision of Some Species Referred to *Leptonereis*, *Nicon*, and *Laonereis* (Polychaeta: Nereididae). By Marian H. Pettibone. Pp. 53. (Washington, DC: Smithsonian Institution Press, 1971. For sale by US Government Printing Office.) 60 cents. [71]

Darwin's Victorian Malady: Evidence for Its Medically Induced Origin. By John H. Winslow. (Memoirs of the American Philosophical Society, Vol. 88.) Pp. 94. (Philadelphia: American Philosophical Society, 1971.) [71]

Organization for Economic Co-operation and Development. Reviews of National Science Policy—Spain. Pp. 123. (Paris: OECD; London: HMSO, 1971.) 11 francs; £0.85. [71]

US Department of the Interior: Geological Survey. Water-Supply Paper 1608-M: Hydrographic and Sedimentation Survey of Kajakai Reservoir, Afghanistan. By Don C. Perkins and J. K. Culbertson. Pp. iv+43. (Washington, DC: Government Printing Office, 1970.) 30 cents. [71]

Science Research Council of Canada. Background Study for Science Council of Canada—Special Study No. 21: Basic Research. By P. Krus. Pp. 73. (Ottawa: Information Canada, 1971.) \$1.50. [101]

US Department of the Interior: Geological Survey. Water-Supply Paper 1908: Surface Water Supply of the United States 1961-65. Part 3: Ohio River Basin. Vol. 2: Ohio River Basin from Kanawha River to Louisville, Kentucky. Pp. viii+581+1 plate. \$3. Water-Supply Paper 1909: Surface Water Supply of the United States 1961-65. Part 3: Ohio River Basin. Vol. 3: Ohio River Basin from Louisville, Kentucky, to Wabash River. Pp. viii+554. \$3. Water-Supply Paper 1935: Surface Water Supply of the United States, 1961-65. Part 14: Pacific Slope Basins in Oregon and Lower Columbia River Basin. Pp. x+957+1 plate. \$4.50. (Washington, DC: Government Printing Office, 1971.) [101]

US Department of the Interior: Geological Survey. Water-Supply Paper 1870-D: Summary of Floods in the United States during 1966. By J. O. Rostvedt et al. Pp. v+99. (Washington, DC: Government Printing Office, 1971.) 50 cents. [111]

Twenty-sixth Annual Report of the Council of the Queensland Institute of Medical Research for the year ended June 30, 1971. Pp. 23. (Brisbane: Council of the Queensland Institute of Medical Research, 1971.) [111]

Annals of the Transvaal Museum. Vol. 27, No. 8: The Southern African Subgenus of *Nudarelia cytherea* (Fabricius, 1775) (Lepidoptera: Saturniidae). By H. Geertsema. Pp. 171-182+1 plate. Vol. 27, No. 9: Neue Lamiinae aus Südafrika (Coleoptera: Cerambycidae). Von S. Breuning. Pp. 183-192. Vol. 27, No. 10: South African Lepidoptera, 5. Revision of the Genus *Tarsoceira*, Butler 1898 and Descriptions of New Satyrinae (Lepidoptera: Nymphalidae). By L. Vari. Pp. 193-224+2 plates. (Pretoria: Transvaal Museum, 1971.) [121]

US Geological Survey. Bulletin 1018-U: Geology of the Near Islands, Alaska. By O. Gates, H. A. Powers and R. E. Wilcox. Pp. vii+709-822+3 plates. Bulletin 1295: Bedrock Geology of Rhode Island. By Alonzo W. Quinn. Pp. v+68. (Washington, DC: Government Printing Office, 1971.) [131]

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